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A CONTRIBUTION
TO THE
NATURAL HISTORY OF SCARLATINA
GRESSWELL

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London
HENRY FROWDE



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A CONTRIBUTION

TO THE

NATURAL HISTORY OF SCARLATINA

DERIVED FROM OBSERVATIONS ON THE LONDON

EPIDEMIC OF 1887-1888

BEING

A DISSERTATION FOR THE DEGREE OF DOCTOR OF MEDICINE
IN THE UNIVERSITY OF OXFORD

*Printed and published at the expense of the University, as a mark of
distinction, in conformity with the Statutes*

BY

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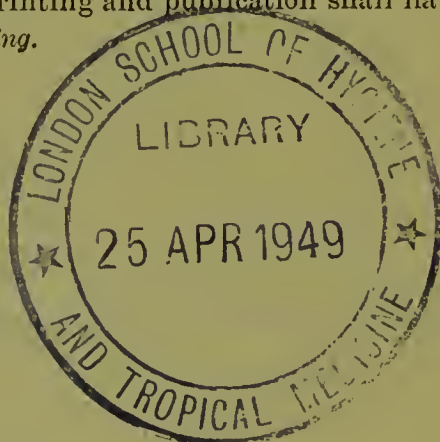
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2. The dissertation shall be delivered to the Regius Professor of Medicine, and shall be submitted by him to and be subject to the approval of those Professors of the Faculty and Examiners for the Degree of Bachelor of Medicine for the time being whose special subjects are dealt with in it.

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4. *A Candidate for the Degree of Doctor of Medicine may offer as his dissertation a work already printed or published if written by him since taking his degree of Bachelor of Medicine and if such printing and publication shall have taken place within the two years immediately preceding.*



INTRODUCTION.

I PURPOSE here to record certain Clinical Observations on human Scarlatina in the Metropolitan District¹, London, as it appeared between September 1887 and February 1888; devoting special attention to its variations in relation to season, and considering in conclusion certain suggestions as to the nature of this relation. I shall not, however, abstain from entering upon collateral issues, which appear to have importance; and, while attempting to bring together phenomena in their natural relations, I shall throughout, as far as I am able, pay regard to considerations which have been advanced by way of explanation in previous times.

The patients, to be referred to, were under my constant and immediate care; a matter for which I am indebted to Dr. P. H. McKellar, Medical Superintendent of the South Western Fever Asylum of the Metropolitan Asylum Board.

¹ This district, known also as Inner London or Registration London, covers an area of eleven square miles, and comprises a population enumerated in the year 1881 at 3,816,483.

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SECTION I.

GENERAL CONSIDERATIONS.

CHAPTER I.

SCARLATINA IN ENGLAND: HISTORICAL.

It was in large or chief part owing to observations on Scarlatina as it manifested itself in England that this disease came to be distinguished from other diseases with which it had previously been confounded. It was Scarlatina as it appeared in London during the years 1661 to 1675 that formed the ground-work whereon Sydenham founded his differentiation of this disease: but, though then differentiated—it had previously been described by Ingrassias and by Sennert¹—it was not until Heberden, Fothergill, Willan, and Withering added their testimony to that of previous writers that the disease came to be recognized as distinct, separate, and specific. The stress laid upon the throat mischief tended, however, to confound Scarlatina with Angina maligna; and it seems doubtful whether this confusion has not continued in England, not only to within registration times, but even up to quite recent years.

Hence, for a trustworthy continuous record of the behaviour of the disease, we are not able to go back very many years; in fact, we are limited to the more recent of the Poor-Law returns, of the reports of the various hospitals into which the patients have been admitted, of those of the Registrar-General, and to the still more recent notification returns for towns where notification has for

¹ After speaking of small pox and measles (in Lib. iv. cap. 12. De febris) Sennert described another form of fever, which was clearly Scarlatina: and it may be noted that in his description of this other form he refers to the articular determinations, which are apt to arise, thus: ‘In declinatione tandem materia ad articulos extremorum transfertur, ac dolorem et ruborem, ut in arthriticis excitat.’

some time been compulsory. The statistics thus ready at hand have furnished means for considering the relation of Scarlatina to age, sex, and season, as also to other diseases: and of the memoirs which have been written on these subjects, mention may be made of those by Messrs. A. Buchan and A. Mitchell¹, by Dr. Matthews Duncan², by the Registrar-General³, by Dr. G. B. Longstaff⁴, by Dr. Barnes⁵, by Dr. B. A. Whitelegge⁶, and by Dr. Ballard⁷, together with those by Drs. Richardson and Murchison.

Statistics of scarlatinal mortality for England and Wales, and separately those for London, for the period since registration of deaths commenced, are given in Tables 18 and 19 of the Registrar-General's Report for 1887. It is necessary, however, to observe that in those tables scarlatinal statistics for the years 1853-58 include Diphtheria and Cynanche maligna; and that for the years 1859-60 they include the latter of these; for though Diphtheria may be closely related to Scarlatina, and Cynanche maligna very probably was Diphtheria⁸, it is necessary to compare such statistics only as are strictly inter-comparable. And in considering death-rates from Scarlatina, it will also be necessary to bear in mind that though they have been related to the prevalence of hospital-scarlatina⁹, as will appear below, they cannot be regarded as necessary indices of scarlatinal prevalences in general; Scarlatina

¹ 'Journal of the Scottish Meteorological Society,' for 1875.

² 'Edinburgh Medical Journal,' for 1876.

³ 'Annual Summary for 1880,' and 'Annual Report for 1886.'

⁴ 'Transactions of the Epidemiological Society,' London, for 1880 and 1885.

⁵ 'British Medical Journal,' November 12, 1887.

⁶ 'Transactions of the Epidemiological Society,' London, 1888.

⁷ Communication to the 'Metropolitan Association of Medical Officers of Health,' about the year 1869, and published in the 'Transactions of the Epidemiological Society,' London, 1888.

⁸ It is true that affections bearing the generic title Cynanche (e. g. Cynanche, Cynanche maligna, Cynanche tonsillaris, Cynanche trachealis), together with others named sorethroat, inflammation of throat, putrid sorethroat, appeared very frequently as causes of death in the Registers before 1855; and that they appeared very much less frequently within a few years afterwards, while diphtheria suddenly came into prominence: but these facts are as suggestive of a mere substitution of names as of any intrinsic difference between the affections referred to. While prosecuting enquiries into diphtheria-prevalences on behalf of the Local Government Board in 1885 and 1886, the conclusion was forced upon me that diphtheria was not a disease new to this country in or about the year 1855; and in passing I may note that diphtheria was registered as a cause of death in Wighton (Norfolk) in 1844, and again in Mansfield in 1845.

⁹ I shall use the term hospital-scarlatina for cases of Scarlatina treated in hospital.

being at one time, as Sydenham found it, scarcely deserving of the name of a disease, and at another, may be, very fatal.

Attention may now be directed to curves I and II of Chart I: they set out, as will be seen, the yearly death-rates from Searlatina in London (curve II), and those for the rest of England and Wales (curve I), per 10,000 of the population, since the year 1862.

The most striking facts revealed by these curves are the periodic exaeerbations and remissions of scarlatinal mortality, the concurrence of these exaeerbations and of these remissions in each of the two areas throughout the period since 1862, and the large aggregate fall of the mortality since 1870.

During the several succeeding quinquenniads commeneing with 1861, and ending with 1885, the scarlatinal mortality in England per million of persons living has been recorded by the Registrar-General to have been as follows, viz. 982, 959, 758, 679, and 434.

As just remarked, however, the fall has not been progressive year by year: indeed, as shown in the Chart, there have been three or four periods, each of one year's to three years' duration, when the mortality has been exceptionally great. Nevertheless the mortality for successive groups of years has fallen remarkably; and in 1885 and 1886 it was lower than it had been since at least 1861, though in the year 1873 the mortality in London fell almost as low. It rose, however, somewhat in 1887.

The similarity of the curves for London and the Provincies is very close, the rises and falls having taken place almost coincidently in the two areas. And on comparing the yearly mortalities during the twelve years immediately preceding, with those during the twelve years following the end of 1875, it is found that in the first period the mortality for London exceeded that for the Provincies on five occasions, that in the second period it was in excess precisely the same number of times, and that on one occasion (1882) the mortality was the same for both areas. The Registrar-General finds, however, that the mean annual scarlatinal mortality per 1000 of the population for the years 1859-70 was for London 1.11, and for England and Wales only 0.93; whereas during the years 1871-84 the proportions were reversed, London having the smaller rate, viz. 0.56, England and Wales having the rate 0.65. And he suggests that the reason of this reversal may have been the establishment

in recent years of the Metropolitan Asylum hospitals for the reception and isolation of scarlatinal patients, no less than a sixth¹ of the registered deaths from this disease in 1884 having, he says occurred in these hospitals, and London being as regards provision for isolation of scarlet fever 'far ahead of the aggregate country.' As bearing upon this it may, however, be mentioned that the mortality from measles in London has been, as pointed out by the Registrar-General, almost invariably above that for England and Wales; it seems indeed that it has been more constantly so of late years: and these facts suggest that compensation as between one disease and another may have been operative in bringing about the differences referred to.

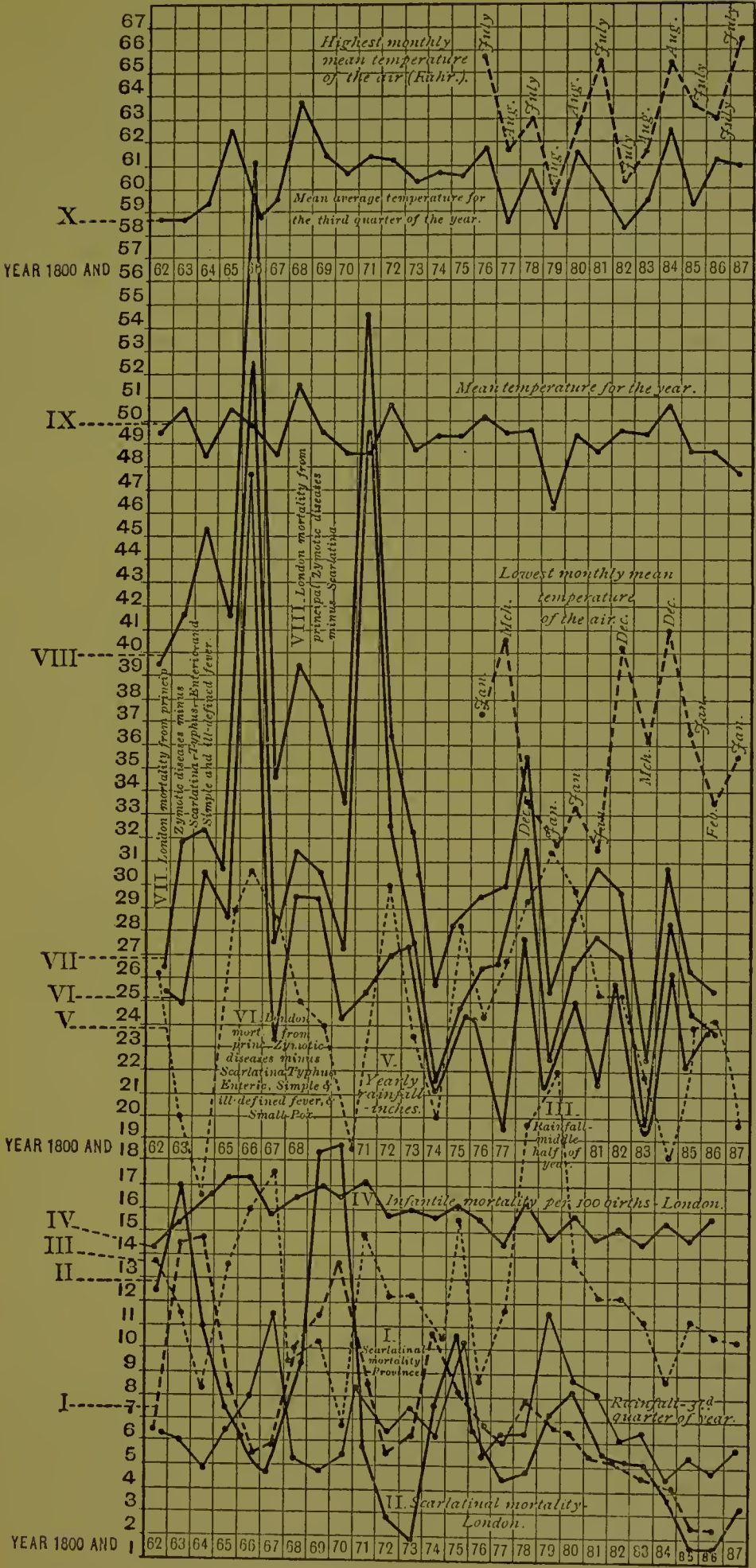
Other questions, arising out of an examination of these curves, will at once suggest themselves; but it will be well to postpone the consideration of them until the relation of Scarlatina to season has been dealt with, reverting to them later, in the concluding section.

Before proceeding, however, to discuss the relation of Scarlatina to season, certain further facts must be mentioned concerning the mortality and prevalence of the disease in London in more recent years, and particularly in the year 1887.

The death-rate from Scarlatina has fallen (v. Chart I) almost yearly in London (as also in the Provinces) since the year 1880, the year 1887 notably excepted. The amount of this disease in a form equivalent in severity to the hospital-scarlatina has been estimated by the Registrar-General. He says in his Annual Summary for

¹ Prior to admission of scarlatinal patients into the Asylum Board's hospitals, in the early part of the year 1872, persons suffering from this disease in London had to be treated for the most part at their homes: some were admitted into the General Hospitals, and large numbers also into the London Fever Hospital. Since the beginning of the year 1872 they have, however, been treated in progressively increasing number in hospitals under the management of the Asylum Board: the London Fever Hospital has continued for most of the time to take in paying patients; but at most of the General Hospitals the wards allotted to fever have been devoted to other purposes. This larger use of the Board's hospitals may be gathered from the following statement made by the Registrar-General. In his Annual Summary for 1887 he says that in 1878 'the deaths from scarlet fever in the London Fever and Metropolitan Asylum Hospitals were only 7 per cent. of the whole mortality from that disease in London; in 1879-82 they were from 9 to 11 per cent.; in 1883 the percentage rose to 13.2; in 1884 to 17.6; in 1885 to 20.2; in 1886 to 22.7; while finally, in 1887, it reached the high figure of 36.6; that is to say, considerably more than a third of the deaths from this disease in 1887 occurred in these hospitals.'

CHART I.—Showing, for the period since 1862, the yearly death-rates (per 10,000 persons living) from certain specified diseases in London (curves II, VI, VII, and VIII), and from scarlatina in the Provinces, i. e. in England and Wales minus London (curve I); also the yearly infantile mortality (per 100 births registered) in London; and certain meteorological data recorded for Greenwich under the direction of the Astronomer-Royal (rain being represented as inches, and temperature as degrees Fahrenheit).



1887: 'Assuming the ease-mortality in the hospitals to represent the ease-mortality in the whole of London, the total number of cases of scarlet fever of equal severity to those admitted into hospital must have been as follows in each of the last eight years: '—

1880 . . . 28,039 cases.	1884 . . . 13,912 cases.
1881 . . . 21,376 „	1885 . . . 8886 „
1882 . . . 23,194 „	1886 . . . 8185 „
1883 . . . 18,748 „	1887 . . . 13,841 „

Hence, with the above proviso, Searlatina of this severity prevailed largely in 1880; it declined yearly in prevalence from 1880 to 1886, with the exception of a slight increase in 1882; and it again prevailed somewhat more abundantly in 1887.

As was said at the outset, it is with the disease in London in this latter year, and the early part of 1888, that I shall in this paper be more especially concerned: and I now proceed to give yet other particulars regarding its general prevalence at those times.

It appears, from the returns of the Metropolitan Asylum Board, that the numbers of scarlatinal patients, admitted into the Board's hospitals in consecutive fortnightly periods during the years 1887 and 1888, were as shown in the following list:—

SCARLATINAL.

	¹ Patients admitted into the Asylum Board's hospitals.	Patients who died in London.		¹ Patients admitted into the Asylum Board's hospitals.	Patients who died in London.
In the fortnight ended Jan. 15, 1887	79	28	In the fortnight ended Jan. 14, 1888	295	62
„ „ 29, „	85	30	„ „ 28, „	255	71
„ Feb. 12, „	84	26	„ Feb. 11, „	241	74
„ „ 26, „	63	30	„ „ 25, „	197	62
„ Mch. 12, „	67	22	„ Mch. 10, „	184	56
„ „ 26, „	65	29	„ „ 24, „	171	49
„ Apr. 9, „	54	13	„ Apr. 7, „	132	60
„ „ 23, „	68	28	„ „ 21, „	183	38
„ May 7, „	128	31	„ May 5, „	149	38
„ „ 21, „	109	27	„ „ 19, „	173	32
„ June 4, „	89	37	„ June 2, „	144	36
„ „ 18, „	88	35	„ „ 16, „	150	42

¹ These numbers have been supplied to me by Mr. Jebb, Clerk to the Metropolitan Asylum Board. Their totals (10,382) do not exactly correspond to the corrected totals (10,366) recorded in the Annual Reports of the Board; the difference is, however, only small; and there is no means of correcting the numbers here given.

	¹ Patients admitted into the Asylum Board's hospitals.	Patients who died in London.		¹ Patients admitted into the Asylum Board's hospitals.	Patients who died in London.
In the fortnight ended July 2, 1887	102	27	In the fortnight ended June 30, 1888	164	29
„ „ 16, „	158	48	„ July 14, „	163	28
„ „ 30, „	182	42	„ „ 28, „	173	37
„ Aug. 13, „	171	46	„ Aug. 11, „	128	35
„ „ 27, „	261	59	„ „ 25, „	138	38
„ Sept. 10, „	373	92	„ Sept. 8, „	162	34
„ „ 24, „	481	95	„ „ 22, „	202	44
„ Oct. 8, „	598	86	„ Oct. 6, „	212	46
„ „ 22, „	602	114	„ „ 20, „	195	63
„ Nov. 5, „	606	129	„ Nov. 3, „	132	49
„ „ 19, „	432	103	„ „ 17, „	148	49
„ Dec. 3, „	393	106	„ Dec. 1, „	139	44
„ „ 17, „	343	77	„ „ 15, „	142	47
„ „ 31, „	249	78	„ „ 29, „	80	45

These numbers show that there was a considerable extension of hospital-scarlatina in the late summer and in the autumn of the year 1887 among those classes of the population that have recourse to the Board's hospitals. It should, however, be stated that there used to be a necessity for making an application to the Relieving Officer in order to gain admission into the Board's hospitals; and that this requirement continued in operation until, but not after, July 8th, 1887. This may have had some influence upon the numbers admitted after that date, so as to render them not strictly comparable with prior admissions: still, the relation borne by the numbers of admissions to the numbers of deaths throughout London in successive fortnightly periods, which are given in the above list, serves to show that the number of admissions does furnish some sort of gauge as to the prevalence in London, i.e. of Scarlatina having severity equivalent to that of the hospital-scarlatina.

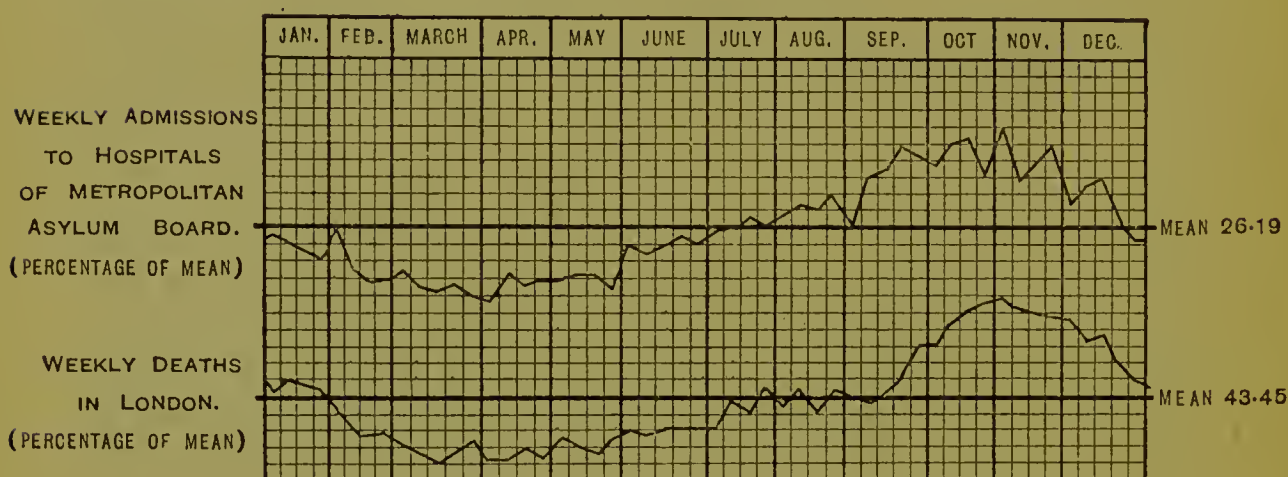
This view is confirmed by the fact that essentially the same seasonal variation of number of deaths from Scarlatina in London, as that shown in the above list, was found by Messrs. Buchan and Mitchell for the 30 years 1845-74, by the Registrar-General for the 40 years 1840-79, and by Dr. Longstaff for the 10 years 1875-84; and also by the fact that essentially the same seasonal variation of rate of admissions of scarlatinal patients into the

Board's hospitals was found by Dr. Longstaff for the latter series of years, as may be seen on reference to the curves, which I have his consent here to introduce from his paper in the 'Transactions of the Epidemiological Society, London, for 1885.' The parallelism between the rate of admissions into the Board's hospitals and the rate of deaths throughout London is also shown by two of the curves in Chart II. p. 21; representing one of them the average daily number

SCARLET FEVER, LONDON¹.

Weekly Average 10 years, 1875-1885.

(Copied from Dr. Longstaff's paper.)



One Division of the Vertical Scale corresponds to 10 per cent.

of admissions, and the other the average weekly number of deaths for consecutive months, the latter being the same periods as those adopted by Dr. Longstaff, some of them being four weeks, others five weeks. The degree of parallelism between these two curves is so close as to suggest that either would serve as a gauge of the ebb and flow of the tide of prevalence of Scarlatina equivalent in severity to the hospital-scarlatina. It will, however, be observed

¹ The thick horizontal lines show the mean weekly number of scarlatinal patients admitted into the Board's hospitals, and the mean weekly number of deaths from Scarlatina throughout London, supposing that both admissions and deaths were equal throughout the fifty-two weeks of the year, the fifty-third week being ignored.

The curves show the amount per cent. by which the actual number of these admissions and deaths exceeded or fell short of these respective means, excess being indicated by parts of the curves above the horizontal lines, diminution by parts below them.

that the death-curve rose while the admission-curve fell in November. In this connection it may be stated that, as I shall show later, the severity of my cases was greatest among persons attacked in October, and the first three and a half weeks or so of November; and that it began to fall off rapidly afterwards. Again, the fall in the death-curve would naturally be somewhat anticipated by that of the admission-curve, and the more so as the former is derived, not from the actual dates of the deaths, but from the dates of their registration. Still it is not clear that these considerations altogether explain the fact that, whereas the ascents of the two curves are closely, their descents are not so closely parallel. The maintained elevation of the death-curve in December 1887, and January, February, and March 1888, may have been due either to abstention from hospital among the classes which had been using the Board's hospitals, or to a greater mortality among classes which do not resort to these hospitals. However this may be, there can be no doubt that the facts portrayed in these curves, confirmed as they are by curves drawn for previous years by other observers, show that from about June 1887 to July 1888 there was in London but one rise and fall, one wave of prevalence of Scarlatina equivalent in severity to the hospital-scarlatina; and that it was at its highest in the last four months of 1887. Moreover, the numbers of admissions from each of the Parishes and Unions of the Metropolitan District show that the wave of prevalence of hospital-scarlatina in these several areas took practically the same course as that which has been depicted for the whole of London, so that the probability that the epidemic was essentially one is thus confirmed. It may be well to mention the numbers of scarlatinal patients admitted into the several hospitals of the Board from Lambeth, Wandsworth, and Clapham, districts to which most of my patients (at least most of those who were attacked in November, December, and January) belonged. The numbers admitted from these districts in successive four-weekly periods, commencing with January 1st, 1887, and ending October 4th, 1888, were as follows, viz. 19, 15, 22, 20, 26 30, 44, 53, 101 (Aug. 11, 1887 to Sept. 8), 184 (Sept. 8 to Oct. 6), 195 (Oct. 6 to Nov. 3), 178 (Nov. 3 to Dec. 1), 165 (Dec. 1 to Dec. 29), 119 (Dec. 29 to Jan. 26, 1888), 87 (Jan. 26 to Feb. 23), 66 (Feb. 23 to Mch. 22), 62, 57, 53, 34, 39, 54 (Aug. 9, 1888, to Sept. 6), and 51.

Seeing, then, that my patients were attacked in the period from September 1887 to January 1888, it will be evident that they belonged to the periods of final ascent, of culmination, and of commencing decline of the epidemic.

I turn now to the scarlatinal mortality in London for 1887. The Registrar-General reported in his Annual Summary that the deaths for that year 'numbered 1447, giving a rate at 0·34 per 1000.' He added 'this rate was higher than in either of the two preceding years, 1885 and 1886; higher also than in the years 1872 and 1873; but, with these four exceptions, was lower than in any single year included in our records of this disease, which go back to 1859 (Table XIII); and 28 per cent. below the decennial average, which was 0·47 per 1000 (Table IV):' so that 'the mortality was thus exceptionally low as compared with that of previous years.' The case-mortality cannot, however, be determined. The number of deaths among the patients admitted during the year into the Board's hospitals is of course not stated in the Annual Report for 1887, for as many as 2050 patients remained under treatment at the end of the year; but it may be remarked that among the total of scarlatinal patients, viz. 6392, treated during the year in the Board's hospitals there was a fatality¹ of 9·54 per cent. as against a fatality in the preceding year (1886) of 9·04.

The scarlatinal patients who came under charge of the Board in 1887 were treated in seven different hospitals (five in London, two in the suburbs). They were admitted 'from every Parish and Union in the Metropolitan District.' 'The Parishes of Paddington, St. Marylebone and Lambeth, and the Holborn Union contributed the greatest number of scarlet fever cases in proportion to their population, and the Poplar, Greenwich, and Woolwich Unions the least numbers': the exact proportions per 1000 of the population in the various Parishes and Unions are given in the bottom line of Table D.

The distribution as to sex and age of these patients is given in Table A, from which it appears that males formed 49·1 per cent. of the whole number.

¹ The fatality is calculated according to the Registrar-General's formula, by dividing the number of deaths multiplied by 100, by half the sum of the admissions, discharges, and deaths for the year.

TABLE A.—*Age- and sex-distribution of the scarlatinal patients admitted into the Board's hospitals during the year 1887.*

Age-groups.	Males.	Females.	Totals.
Under 5 years	835	815	1650
5 to 9 ...	1212	1265	2477
10 to 14 ...	466	502	968
15 to 19 ...	218	197	415
20 to 24 ...	84	113	197
25 to 29 ...	45	45	90
30 to 34 ...	23	27	50
35 to 39 ...	11	15	26
40 to 44 ...	5	9	14
45 to 49 ...	2	5	7
50 to 54 ...	1	5	6
And upwards
Totals	2902	2998	5900

Most of the admissions to hospital took place, as already shown, in the latter part of the year: those admitted during the last five months having formed 75·8 per cent., and those admitted during September, October, and November 55·7 per cent. of the total number admitted. The maximum number of scarlatinal patients under treatment on any one day was reached on November 23rd, the number then being 2611.

It has been indicated above that the abolition of the necessity for obtaining an order from the Relieving Officer may have resulted in increase of the number of persons admitted into the Board's hospitals afterwards: it may have resulted also in attracting to the hospitals persons in better circumstances; but it would appear that there was no sensible change in this respect, the patients under the charge of the Board having been in chief part, as previously, of the poorest of the London population.

CHAPTER II.

THE PATIENTS SPECIALLY REFERRED TO IN THIS PAPER, AND THE CONDITIONS UNDER WHICH THEY WERE PLACED.

It has already been said that in the course of the two years 1887 and 1888 there were in all 10,366 scarlatinal patients admitted into the Asylum Board's hospitals. Of those admitted between early September, 1887, and early February, 1888, I had sole medical charge of 588; and it is to these that this paper will be more especially devoted. Several general matters regarding these patients may now be mentioned; but it should first be stated that they were not altogether consecutively admitted, a matter to which I shall give full consideration later.

The age and sex of my patients, and the *months* in which they were severally *attacked* are set out in Table B. Males formed 52·5 per cent. of the whole number. The patients attacked in November

TABLE B.—*Showing the distribution of attacks according to season, age, and sex.*

Month in which attack occurred.	Under 1 year.		1 and 2 years.		3 and 4 years.		5 to 9 years.		10 to 14 years.		15 to 19 years.		20 to 24 years.		25 to 29 years.		30 to 39 years.		40 and 41 years.		At all ages.		Totals.
	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	
August cases	0	0	0	0	2	1	0	1	0	1	0	0	0	0	0	0	0	0	0	0	2	3	5
Sept. „	0	0	11	10	32	17	41	33	11	22	4	7	3	3	8	1	0	2	0	0	110	95	205
Oct. „	0	0	9	9	16	10	31	34	18	12	12	10	4	5	1	3	1	1	0	0	92	84	176
Nov. „	1	1	8	1	9	5	12	7	7	10	3	4	1	2	1	1	3	0	1	1	46	32	78
Dec. „	1	0	0	2	12	10	13	11	5	4	8	1	3	2	3	0	3	1	0	0	48	31	79
Jan. „	1	0	3	4	5	5	0	11	1	8	0	4	0	1	0	1	1	0	0	0	11	34	45
	3	1	31	26	76	48	97	97	42	57	27	26	11	13	13	6	8	4	1	1	309	279	588

and December were almost precisely the same in number; and the proportion of males to females, as well as the proportion of patients under 5 years of age, were almost the same for November as for December. I draw attention especially to these months for reasons which will appear below.

The stages of illness on admission, as nearly as could be ascertained, are given in Table C. More than 90 per cent. of the cases were admitted on or before the eighth day of illness: 83 per cent. were admitted between the first and sixth day. On comparing the November and December cases it will be observed that, though there is some disparity in the proportions of cases admitted on or before the fourth day, there is but little in the proportions of cases admitted on or before the eighth day. The January cases were, it seems, brought into hospital at a considerably earlier stage of illness than the other cases.

TABLE C.—*Showing day of illness on admission.*

Month in which attack occurred.	1st Day.	2nd Day.	3rd Day.	4th Day.	5th Day.	6th Day.	7th Day.	8th Day.	Later.	Totals.	Percentage of cases admitted on or before the	
											4th Day.	8th Day.
August cases	0	0	0	0	0	0	0	0	5	5	0	0
Sept. „	0	36	69	52	23	1	2	5	17	205	76	91
Oct. „	2	35	65	23	20	5	10	5	11	176	71	93
Nov. „	3	17	27	13	10	4	1	0	3	78	76	94
Dec. „	2	11	21	19	8	6	5	1	6	79	67	92
Jan. „	1	15	15	9	4	0	1	0	0	45	88	100
	8	114	197	116	65	16	19	11	42	588		
83% of the total No.												

The Parishes and Unions, to which the monthly sets of patients severally belonged, are shown in Table D. The majority of the patients belonged to the Lambeth Parish, and Wandsworth and Clapham Union, both in the county of Surrey; and the November and December patients were taken very largely, and in not very unequal proportions, from these districts.

[Some of the *meteorological data* for the year 1887 and for part of 1888, as recorded for Greenwich under direction of the Astronomer Royal, are graphically presented in Chart II and in Tables Y^I and Y^{II}. The mean monthly temperature of the air and of the dew point, and the average number of hours per week of bright sunshine rose to a maximum for the year 1887 in July, and each fell month by month afterwards. In passing it may be noted that the average per month of deaths weekly from Scarlatina in London began to rise in May, fell slightly in June, rose again in July, further in August,

TABLE D.—Showing the localities to which the patients attacked in the several months belonged. It also shows in the bottom line the proportion of the total number of cases of scarlet fever admitted from each locality into the hospitals of the Asylum Board throughout the year 1887 per 1000 of the population.

Months in which attack occurred.	Lambeth.	Wandsworth and Clapham.	St. Saviour's.	Camberwell.	Greenwich.	Chelsea.	St. George's-in-the East.	Kensington.	St. Olave's.	Westminster.	Fulham.	Strand.	Bloomsbury.	Lewisham.	Paddington.	Marylebone.	Shoreditch.	City of London.	Stepney.	Bethnal Green.	Holborn.	Poplar.	Mile End.	Islington.	Totals.	Percentage of cases from Lambeth, Wandsworth, and Clapham.
August cases	1	1	0	0	0	0	0	1	1	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	5	40
Sept.	56	33	30	5	0	4	13	6	14	1	2	0	0	0	4	5	7	2	2	0	11	2	1	1	205	43
Oct.	51	47	18	15	3	1	4	11	10	3	8	0	2	2	1	0	0	0	0	0	0	0	0	0	176	55
Nov.	36	33	3	2	2	1	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	78	82
Dec.	44	33	1	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	79	97
Jan.	21	12	0	0	0	2	3	1	0	2	3	1	0	0	0	0	0	0	0	0	0	0	0	0	45	73
Admissions per 1000 of the population into all the hospitals of the Board in the course of the year 1887.	2.44	2.00	1.97	1.43	0.64	1.8	1.75	1.9	1.26	1.84	1.87	1.72	2.02	0.7	2.15	2.42	1.54	1.34	1.26	1.34	2.62	0.47	0.91	0.92		

still further and more rapidly in September, then slowly to the maximum for the year in November, after which it almost as quickly fell, but not even in the next thirteen months to the level for April 1887. And the curve of average daily admissions per month into the Board's hospitals ran, as has been said, with marked approach to parallelism to the curve of mean weekly deaths. The rainfall for 1887 and for 1888 is given with that for other years in Chart I.]

The conditions under which the patients passed through their illnesses after their removal from home may now be briefly reviewed. The patients were all conveyed to hospital in the recumbent posture, in specially appointed vehicles belonging to the Board, over a distance averaging perhaps two miles. After verification of the diagnosis in the Receiving-room, where they were *clothed* in cotton bedgowns, and over these in flannel, they were transferred to one of the wards, and at once put to bed, the bed consisting of iron bedstead with feather-bed, blanket and sheet under the patient, and a sheet, three blankets (with additions when desired, as was commonly the case with adult males) and counterpane above. The *wards* occupied are ten long stoutly-built brick two-storeyed structures, with internal walls either covered by Parian cement, or formed of glazed bricks: their height is 14 feet; eight of them have severally a cubic space of 39,000 feet, and two of them a cubic space of 30,000 feet. The floor space allotted to each patient was about 130 feet, and the cubic space (if the full height of the ward be included) about 1700 feet. The *temperature* of the wards was maintained as nearly as possible at 60° F.: it was very rarely higher, but in colder weather it did on occasion fall as low as 45° F.: and, when the outside temperature fell to near the freezing point, it was with difficulty that, free ventilation being allowed, the temperature was maintained constantly above 55° F. *Warmth* was supplied by coal fires in large open grates (two towards either end of the ward, the chimney, of brick or iron, passing nakedly upward in the ward), and by hot water-pipes carried round the ward just above the floor-level. *Ventilation* was at all times ample, free air-exchange being ensured by the upper fourth of each alternate window being kept open, sloping inwards towards the ceiling, and by large openings through the walls immediately above the floor-level. *Daylight* was subdued by common brown-holland window

blinds kept down, a measure ¹, I think, of some value in the treatment during the pyrexial stage. The patients were *confined to bed*, all of them, for a minimum period of twenty-two days following upon admission; and all, save those who were transferred (as soon as thought prudent after being allowed to get up) to the Convalescent Hospital at Winchmore Hill, remained in hospital at least eight weeks. *The diet* during the first twenty-two days after admission consisted of milk, eggs, and half a pint of beef tea ² daily, the milk practically in any quantity. *Baths* were not given during the first three weeks while in hospital, except under special circumstances. They were administered at a temperature of 98° to 100° F. in instances of pyrexial sleeplessness and delirium, in instances of cutaneous itching attending the eruptive stage, and in a few instances in which desquamating epidermis did not fairly readily come away. Beyond this, bathing was not adopted during the first three weeks in hospital. After a patient, however, had been allowed to get up for three days, warm baths were given every evening immediately before going to bed. *In no case was any medicament applied to the surface of the body*, save for a few applications of mustard over enlarged cervical glands. *Medicinal treatment* was in chief part directed to regulating the bowels by means of Colocynth, Castor oil, and the tinctures of Catechu and Kino. Save in exceptional cases medicine for other purposes was not given unless it were as a placebo, two or three drops of Spiritus Chloroformi to three ounces of water in the day. Oxygen inhalations and oxygenated water were, however, freely administered to eight October and four November patients, as recorded in the *Practitioner* for October and November, 1888, but, if at all, only scantily to

¹ In the 'British Medical Journal' of July 26, 1884, I showed that sudden exposure of the eyes to tropical light caused an acceleration of the pulse at the rate of as many as 300 beats per hour in a healthy man aged twenty-one years. Daylight of our latitudes may also perhaps enhance the acceleration of the pulse of a febrile person. Certain it is that some scarlatinal patients while febrile greatly prefer to have the light of day subdued, though there be no perceptible conjunctivitis.

² The beef-tea for each patient's daily ration was prepared from $\frac{1}{2}$ lb. of meat with bone; and it is, I think, of importance to mention that it was strained, so that the muscle- and bone-extractives would be relatively much in excess of the proteid matters. I have always looked with apprehension upon the use of strained beef-tea for this reason; and there is the greater room for such apprehension, when it is administered to persons suffering from pyrexia, especially when there is a tendency to renal determination, or to depression of the heart's action.

any others, except it were after getting up. Port wine and brandy were also given in cases of weakness of the heart's action, softness of pulse, lividity of surface, or like condition.

Full consideration will be given in due course to these variations in treatment, and also to the fact that my patients were not altogether consecutively admitted, as affording possible explanation for the variations in the clinical statistics.

SECTION II.

CLINICAL BEHAVIOUR OF THE PATIENTS UNDER OBSERVATION, IN
RELATION TO SEASON; MORE DETAILED CONSIDERATION OF
ALBUMINURIA BEING RESERVED FOR SECTION III.

CHAPTER I.

INTRODUCTION.

IN this section I shall take up the relation to season of some of the main clinical features of my cases; albuminuria being, however, more fully entered upon in Section III. I shall show that after a certain date the symptoms, which had up to that date evidenced a general progressive increase of relative frequency and intensity, remarkably ameliorated; and that this amelioration commenced definitely in the latter part of November. It will be borne in mind that the epidemic, in so far also as prevalence (as judged by numbers of cases admitted to hospital and of deaths) was concerned, reached its acme in October or November; the numbers of admissions into hospital, and of deaths throughout London, having rapidly increased till then, and having rapidly diminished afterwards.

The general relation to season of the behaviour of the disease may be seen at a glance on referring to Table E and Chart II, the latter representing graphically, with other matters, the percentages shown in the former. The statistics for August, relating as they do to but a small number of patients, will not be available for purposes of comparison. The same is true also to some extent of the statistics for January; and though I shall for the present pass these by, it will be necessary later to see whether they affect the conclusions drawn from the earlier cases. In the bottom line of Table E may be read the general clinical features of my cases:

it is not, however, with them that I have here specially to deal ; it is rather with the monthly sets of statistics, in their relation one to another. Examination of the table or of the chart just mentioned reveals the fact that, though in some points the clinical statistics of the September, October, and November cases were much alike, the relative incidence of many complicating conditions increased from September into October or November, and fell away suddenly in December. Confining our attention to the three former months, it may be observed that cervical adenitis, cervical abscesses, tympanitis (otitis media), rashes (not infectious nor contagious), facial dropsy of renal origin, and marked slowing of the pulse, were relatively most common among the October cases ; whereas delirium, the graver conditions of tongue, very marked desquamation and deaths were relatively most common among the November cases. On turning now to compare these statistics with those for December, a notable change is apparent. It will suffice to compare the November and December cases : and in making the comparison let it be noted that, as already shown, the numbers attacked in these two months were approximately the same ; that the patients came in chief part from the same localities ; that in regard to age there was a slight excess of patients under ten years among those attacked in December ; and that stimulants were administered to much the same extent among both sets of patients ; and yet the deaths among the December cases numbered but little more than half of those among the November cases ; delirium was not one-third as common among the former, early dysphagia not two-thirds as common, the graver conditions of tongue and early diarrhoea but little more than half, arthritis only about one-fourth part as common, very marked desquamation not quite, and albuminuria but little more than three-fourths as common.

The graver manifestations, in fact, became relatively more frequent, and I may add more marked, from September into October or November ; and then they suddenly fell off ; not merely in regard to two or three determinations, but in regard to most of them ; —changes which were also expressed in the monthly mortality. It will, however, be requisite to inquire whether such conditions as age, sex, treatment, or other, had any influence in bringing about these results : and I shall now proceed to give evidence on this subject, dealing with each of the main symptoms in succession.

TABLE E.—Presenting certain clinical and other statistics concerning the 588 patients referred to in this paper.

Month in which attack occurred.	Admitted.	Died				1 Percentages of patients affected with																				Percentage of patients treated with port wine or brandy.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																								
		Totals.	At ages under 10 years.	Discharged.	2 Transferred.	Within 48 hours of admission.	At ages under 10 years.	Totals.	Percentages.	At all ages.	within the first				3 Very marked desquamation.	4 Albuminuria.	5 Passage of hæmoglobin or red blood corpuscles into the urine.	Facial dropsy—renal.	Renal convulsions.	Inflammation of middle ear.	Cervical adenitis.	Abscesses requiring to be opened.	Ongchia.	Secondary sore throat not apparently attributable to chilling.	Cutaneous rashes, not infectious nor contagious.		Marked slowing of pulse at any time during first 3 weeks of illness.	Within 3 days of admission.	Later than the 3rd day after admission.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																					
											Delirium	Marked coryza	Marked dysphagia	Diarrhoea																Graver conditions of tongue.	Arthritis, though never previously affected with it.	3	2	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

¹ In the calculation of each percentage those patients, who were not examined (and the numbers of these are shown in the tables that follow), have been carefully excluded.

² Removed, after being allowed to get up, to the Convalescent Hospital at Winchmore Hill.

³ Some of the patients, wholly unselected, were not examined as to these particulars.

⁴ Excluding those who exhibited this symptom on admission, and those in whom it was apparently attributable to respiratory embarrassment.

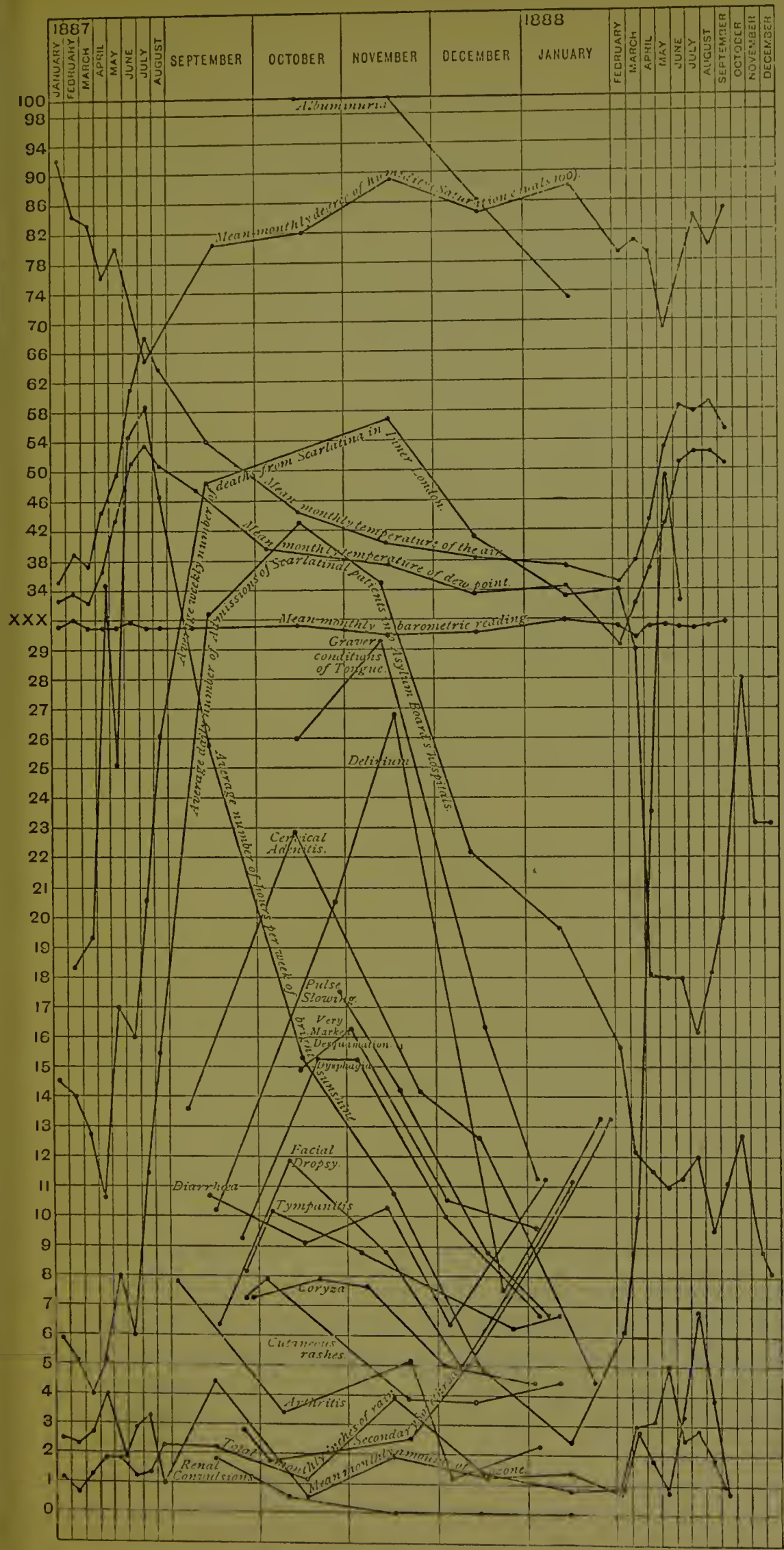


CHART II.—Showing, for each successive month, the numbers both of deaths from Scarlatina in London and of scarlatinal patients admitted into the hospitals of the Asylum Board; also several of the meteorological data recorded for Greenwich under the direction of the Astronomer Royal; and, for the several groups of my patients who were attacked in successive months, the percentage proportion of those who exhibited certain signs or symptoms.

It will be noted that the intervals above the numeral 'XXX' in the Chart have four times the value of those below.

CHAPTER II.

DELIRIUM.

It will be observed, on referring to Table E or Chart II, that the percentage of patients who were delirious within the first eight days of illness increased continuously through September, October, and November; and that it fell in December to a lower level than in any of the preceding months. On turning to Table F, columns III to VI, it will further be seen that, with unimportant exceptions, the rise and the fall in the percentage of delirious patients, here referred to, occurred alike among patients over and patients under ten years of age, and this too among both males and females. In regard to influence associated with sex, it appears that among patients over ten years of age there was a larger proportional incidence of delirium in each month except December among the males. And further in regard to age, it may be noted that there was a larger proportion of delirious cases among males over ten years of age than among males under that age. I was inclined to attribute this to alcoholism, and greater mental strain at the higher age: but it needs to be noted that with increase of age there is not the same readiness to lie up when indisposed; and so it may come about that a larger proportion of delirious patients at the higher ages are sent into hospital, a matter which needs also to be borne in mind in connection with other symptoms.

Allowing, however, fully for the influence associated with age and sex on the manifestation of delirium, there is overruling both, and clearly discernible in Table F, the pronounced factor of season. With unimportant exceptions, the percentage of delirious patients among different age-groups was greatest in November, there having been an increase up to that month and a decrease afterwards. The influence exercised by season is moreover expressed in another manner; for, as shown in Column VII, the proportion of patients, who were delirious till the eighth day of illness or

who died before that time (and in that event also delirious), rose till and fell after November. And again columns IX, X, and XIII

TABLE F.—*Presenting numbers and percentages of patients delirious within the first eight days of illness.*

Nos. of delirious patients; total numbers of patients of corresponding age and sex; and percentage of former to latter.		At ages 1 to 10 years.		At and over 10 years of age.	
		Males.		Males.	
		Females.		Females.	
No. of delirious patients.	Per-centage of delirious patients.	III.		V.	
		IV.		VI.	
September cases	I.	6 in 81, i.e. 7.4°/o	11 in 63, i.e. 17.4°/o	3 in 26, i.e. 11.5°/o	0 in 35, i.e. 0.0°/o
October	II.	12 in 56, i.e. 21.4°/o	5 in 53, i.e. 9.4°/o	9 in 36, i.e. 25.0°/o	4 in 31, i.e. 12.9°/o
November		7 in 29, i.e. 24.1°/o	5 in 13, i.e. 38.4°/o	5 in 16, i.e. 31.2°/o	2 in 18, i.e. 11.1°/o
December		1 in 25, i.e. 4.0°/o	2 in 23, i.e. 8.6°/o	2 in 22, i.e. 9.0°/o	1 in 8, i.e. 12.5°/o
January		0 in 8, i.e. 0.0°/o	2 in 20, i.e. 10.0°/o	1 in 2, i.e. 50.0°/o	3 in 14, i.e. 21.4°/o

No. of patients who died before, or were delirious till, the 8th day of illness.		No. of delirious patients who were affected with					were admitted on or before the 4th day of illness.
		Died.	Tympanitis.	Cervical Adenitis.	Diarrhoea.	Arthritis.	
VII.		VIII.	IX.	X.	XI.	XII.	XIII.
September cases	10, i.e. 4.8°/o	12	3	8	10	1	19
October	13, i.e. 7.3°/o	10	6	11	8	0	21
November	7, i.e. 8.9°/o	7	4	3	4	1	19
December	4, i.e. 5.0°/o	3	1	0	2	0	4
January	1, i.e. 2.2°/o	3	0	0	2	0	6
		20 of 22 patients examined, i.e. 90.9°/o					
		10 of 17					
		2 of 5					
		2 of 5					

show that tympanitis, cervical adenitis, and marked albuminuria among the delirious patients attacked in December were, each of

them, relatively more rare than among those attacked in either October or November. It may be asked whether these, or other complications, noted in Table F, bore causal relation to the delirium. It was not so, for delirium was evidenced in most cases before any of these conditions, save cervical adenitis, had appeared: moreover, in all there were but sixty-four instances in which these conditions did appear; and some of them occurred together in the same individual; whereas the total of delirious patients was eighty-four: early delirium in fact was not a mere symptom of any of the complications mentioned. Stage of illness on admission may also be thought of as possibly having had to do with these statistics; but this suggestion is dispelled by the figures of Column XIV, which show that most of the above delirious patients were admitted on or before the fourth day of illness. Stimulants cannot have influenced the results tabulated in Column II, for in no case were any given before the delirium was manifested. Neither can the administration of oxygen have had any influence in bringing about the increase of delirium in the October and November, or the decrease in the December cases; for the delirium had appeared in each case before oxygen was used.

I might mention here that arthritis in this epidemic (as was also the case in the London epidemic of the years 1881 and 1882) was quite a mild affection; only two of the delirious patients suffered from it, and both they, as also all other patients who had arthritis, recovered; but, it should be added, arthritis as compared with delirium was a comparatively late manifestation of the disease, as it was also in the years 1881 and 1882.

CHAPTER III.

TEMPERATURE.

SEEING that the delirium of Scarlatina is generally associated with a considerable elevation of temperature, and that its relative frequency increased progressively (as it were by leaps and bounds) through September, October, and November, and markedly fell away in December, it may reasonably be surmised that similar changes were to be found in the temperatures. Though able to affirm this in general terms, and definitely of the patients who died, I am not in a position to substantiate it in reference to all the cases by appeal to figures. I find, indeed, that the number of recorded cases comparable in this respect is very limited. There is, moreover, difficulty in fixing upon statistics serviceable for dealing with the subject thus presented. Is it to be, for instance, the maximum temperature reached during the febrile period, the temperature at a certain stage of that period, or the duration of temperature at or above a certain elevation? No doubt regard should be had to as large a field of data as possible, allowance being made for influences incidental to age, sex, family relationship, and so on. I am not, however, aware of any record on a sufficiently large scale which would authorize definite statements as to the intensity and duration of the febrile period in Scarlatina among patients of different age-groups, in males and in females. The protean character of Scarlatina indeed forbids formulation of any such statements, the febrile period varying in duration and the fever in intensity, it seems, with age, with sex, with constitution, with season, most of all with the epidemic. Nevertheless, I submit such comparable statistics as I was able to obtain; they are not numerous, because something in so many cases intervened to prevent a perfectly regular and trustworthy temperature-registration. I have taken, as a datum for testing the influence of season on temperature, the day of illness on which the temperature first

became normal, morning and evening. Of the patients whose temperatures were carefully and regularly taken for several weeks morning and evening, and who had been admitted to hospital not later than the seventh day, I am limited to those under twelve years of age. Of these there were only eighty-eight, viz., twenty-four who were attacked in November, thirty-four in December, and thirty in January. In regard to others attacked in these several months, it is necessary to say that want of regularity in registering the temperature was due to forgetfulness or actual inability to take it twice (at 8 a.m. and 7 p.m.) each day: it was in no way owing to mildness of attack; in other words, no particular class of case was selected for thermometric observation. I mention this with the view of emphasizing the fact that the patients about to be referred to were in no way whatever selected.

The temperature, taken in all instances at approximately the same times, was normal, or, I should say, was below 99° F., both morning and evening, for the first time,

among the 24 Nov. cases in 12.1 days from the beginning of the illness as an average

„	34 Dec.	„	11.1	„	„	„	„
„	30 Jan.	„	9.8	„	„	„	„

As already said, not one of these patients was admitted later than the seventh day of illness; and it appears that of the whole number (eighty-eight) of them as many as eighty were admitted on or before the fifth day. Age and sex cannot be supposed to have largely, if at all, influenced the above results; for the patients, all of whom were under twelve years of age, were distributed very uniformly as regards age and sex, as they were also in regard to stage of illness on admission to hospital. In none was the temperature above normal for more than eighteen days; so that the monthly differences cannot be attributed to inclusion of cases of exceptional severity. And not one of these patients was treated with stimulants or with oxygen.

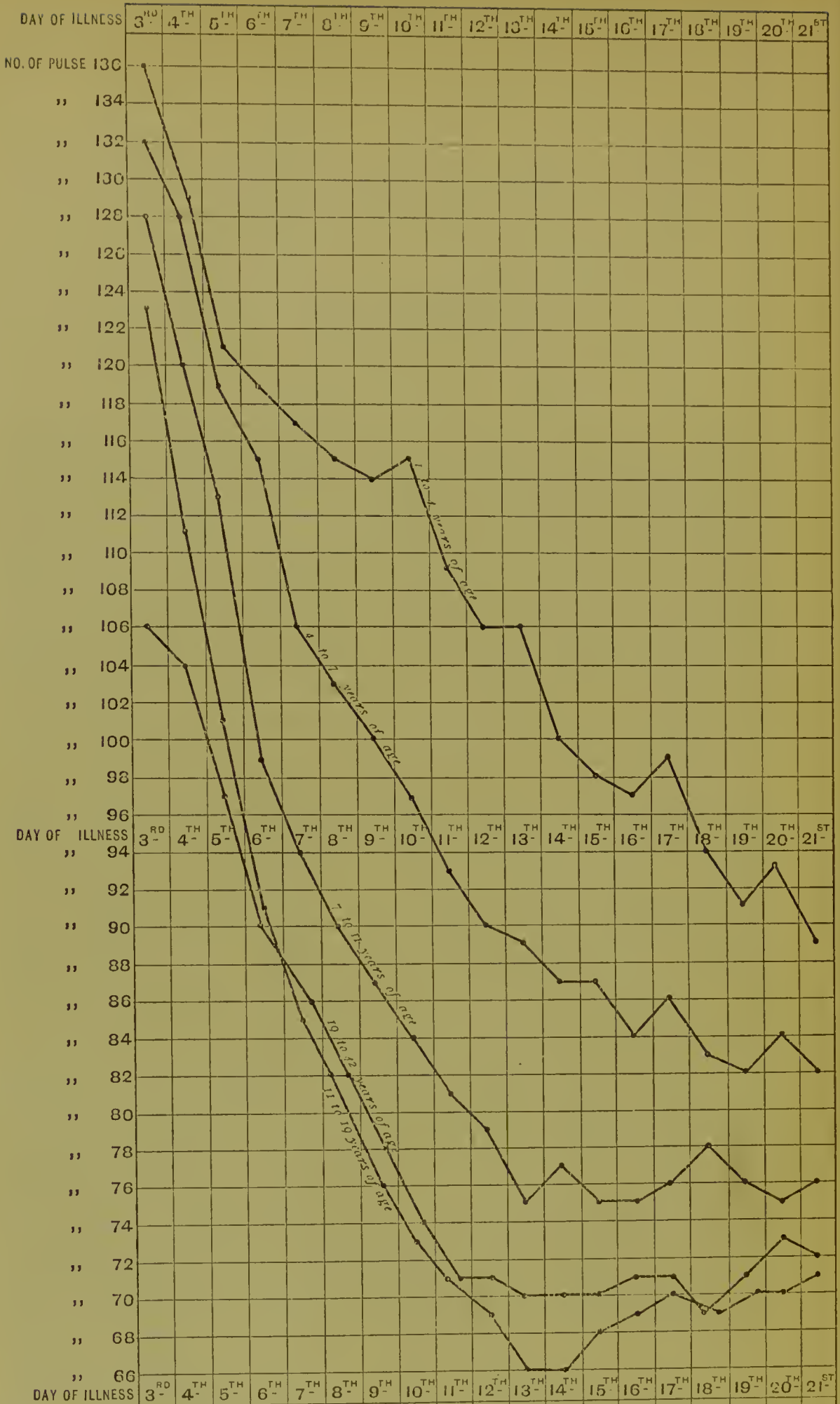
I am not prepared now to give more complete observations on the febrile period, not having yet had opportunity for examining the many complicating cross-relations, which would need to be considered were an exhaustive exposition of the subject attempted.

CHAPTER IV.

PULSE-RATE.

WHILE in charge of a considerable number of scarlatinal patients in the London epidemic of 1881 and 1882, also at the South-Western Fever Hospital, it appeared to me that a continuous record of the pulse-rate was an aid of great value in treatment and prognosis: and on the more recent occasion I continued my observations in this direction, with the view of determining whether any definite rate of pulse for any given day of illness, and any given age, would be of prognostic value. I took the pulse of most of the patients attacked in the period October to January almost every morning for at least the first twenty-one days after admission: and in Chart III. I have graphically represented the pulse-rates thus obtained. The Chart takes cognizance of almost all these patients over the age of one year who recovered. It excludes all those who died, also one patient attacked while suffering from acute rheumatism, and another affected with long-continued suppuration. It having become apparent that sex, age, and season had each their respective influence upon the pulse-rate, I divided the above patients into two groups, according as they had fallen ill in either of the periods—October with November, or December with January; and I subdivided them according as they belonged to one or other of five different age-groups. The average pulse-rate on successive days of illness for each sex in each of these ten groups was next determined; then the mean of the averages (thus obtained) in each of the ten groups for both sexes conjointly; finally, the mean of the means (thus obtained for each age-group and for both sexes together) in the two periods above named: and the resulting numbers are represented in the curves of Chart III. By aid of these curves, coupled with curves of pulse-rate for individuals, it may, I think, be safely affirmed that the pulse of those of the scarlatinal patients who were progressing favourably to health

CHART III.—Showing for groups of persons of different ages (the groups together including all but two of those who recovered among the October to January patients) the mean pulse-rates on consecutive days of illness—each curve showing the mean of two average pulse-rates, one for the males the other for the females belonging to the group to which the curve refers.



Number of patients from whom the several averages were estimated.

UNDER 4 YEARS OF AGE	13	19	26	32	30	33	37	36	35	33	38	25	33	34	37	36	34	34	34
4 TO 7 " " "	13	37	55	66	68	71	75	73	75	76	75	77	75	75	76	76	76	75	72
7 TO 11 " " "	16	37	54	57	63	63	65	62	66	67	62	63	67	68	66	67	67	66	65
11 TO 19 " " "	14	39	51	58	62	64	66	66	67	68	67	67	68	68	65	68	70	67	67
19 TO 42 " " "	7	20	27	32	30	35	36	39	35	40	36	33	42	35	41	40	39	35	39

reached its maximum rate on the first, second, or third day, generally on the second; that it then fell straightway, and, with slight variations, regularly day by day till at least the ninth, and at most ages till the eleventh, in three age-groups till the thirteenth day; after which it continued to fall slowly to normal, or having reached a subnormal number, it resumed, after oscillating, its normal frequency. The pulse, in early stages of Scarlatina, is known for its rapidity. Dr. Ozanam, indeed, asserts that Scarlatina may be diagnosed before the appearance of the rash by the association of sore throat with marked acceleration of the pulse. If the pulse did not fall from the second or third day, and measurably each day, or at least every other or third day; if it were maintained at the same elevation above the normal for more than two or three days; and *a fortiori* if its fall, prior to a subnormal fall, were broken by a rise, there was generally some readily discoverable cause—tympanitis, exaggeration of the tonsillitis, cervical adenitis, cervical cellulitis, arthritis, nephritis, pneumonia, or some other localized mischief. The pulse, taken with due regard to factors (external and internal) modifying it, in recovering and almost uncomplicated cases fell with considerable uniformity to normal; and if an approximation to such fall were not observed, there was some cause demanding inquiry; the falls on the earlier days were, as may be seen, much larger than those for later days of the illness. Mental disturbance is an active factor in modifying the heart's action, and especially so I have thought in Scarlatina; but this emotional pulse is readily detected. As will be shown later, there were several cases in which the pulse was very slow: these are included in the Chart, and they therefore tend to reduce somewhat the height of the curves.

I have already said that age, sex, and season had each their respective influence on the pulse-rate. The influence of age is not very great. It may, however, be remarked that whereas there was but little difference between the pulse-rates for different age-groups on the second day; there was for later days a difference, which speaking generally may be said to have increased up to about the eleventh day, the younger age-groups up to eleven years naturally having the higher averages. The pulse also reached its minimum rate earlier in the older patients; the minimum for the first twenty-one days was on the twenty-first day for ages be-

tween one and four years, on the nineteenth day for ages between four and seven years, on the thirteenth day for higher ages: and recovery from subnormal rates was quickest at ages from eleven to nineteen years, less quick at ages from nineteen to forty-two, and least so at ages from one year to eleven years. As regards sex, it appears that at most ages, and at most stages of the illness, females had higher pulse-rates than males. Taking the average pulse-rates (for successive days of illness) of males and of females, belonging to the different age-groups, and attacked in one or other of the two periods mentioned above (October with November, or December with January), there were altogether 188 rates for males and the same number for females that admit of being compared, one set with the other. In eight instances the rates were the same. In forty-nine instances the rates for the males exceeded those for the females. In the remaining 131 instances, however, the average pulse-rates of the females were the higher. The pulse of the females was below that of the males—in later stages of the illness at ages eleven to nineteen years in the December and January cases, at ages four to seven years in the October and November, and in the early stages at ages from four to seven years in the December and January cases. At all other ages, and stages of illness, the pulse of the females was higher than that of the males.

Over and above the influence of age and sex, there was also the influence of season. With the data already furnished as to frequency and continuance of delirium, and as to continuance of elevated temperature, it may be surmised that a *high* pulse-rate was relatively more frequent in the patients attacked in October and November than in those attacked later. It was so. It is almost needless to remark that this was the case with those who died, with those who were delirious, or those whose average duration of febrile period has already been adduced in testimony of the influence of season. *Slowing* of the pulse also, especially in the later stages of the first three weeks, revealed a seasonal influence. This is shown in Table E, and Chart II. I have examined this matter in further detail among the males; slow pulse-rates among females having been more rare than among males. I have taken as a slow pulse one which, for four or five consecutive days at least of the first twenty-one days of illness, was at or below the numbers as follows, viz. fifty-seven for ages nineteen to forty-two years, fifty-

four for ages eleven to nineteen years, sixty-three for ages seven to eleven years, seventy for ages four to seven years, and seventy-seven for ages one to four years: these several pulse-rates being the lowest averages shown in Chart III for these several age-groups (viz. 69, 66, 75, 82, and 89) minus 12. The results are expressed in Table I; they show for most ages a larger percentage of slow pulses among patients (i.e. male patients) attacked in October and November, and especially among those of ages 4 to 19 years. There are, however, two kinds of slow pulse in Scarlatina; that which is, and that which is not, associated with high arterial tension. Seeing, also, that patients of ages 4 to 19 suffered most, as will be shown in Section III, from albuminuria, and that albuminuria was most frequent among the October and November patients, it may be suggested that renal changes had to do with this seasonal distribution of slowness of pulse. However this may be, arterial tension would appear to have had nothing to do with it.

TABLE I.—*Showing percentages of male patients, whose pulse-rate was 'slow.'*

	1 to 4 years.	4 to 7 years.	7 to 11 years.	11 to 19 years.	19 to 41 years.
October and November cases	0.0°/o	18.7°/o	48.0°/o	72.7°/o	38.8°/o
December and January cases	11.1°/o	10.0°/o	28.5°/o	11.1°/o	33.3°/o

The slow persistent pulse of arterial tension, as slow as the standards used above, was not in any single case reached until a later period of illness than that covered by the above estimates: and, I may add, arterial tension, when associated with such slowing of the pulse as that above spoken of, was always quite marked and most readily appreciated. Mahomed, moreover, who insisted most on the significance of the hard pulse, showed in his second paper on this subject that even hæmorrhagic nephritis with dropsy is by no means necessarily connected with a pulse of raised tension: and it may be said, in general terms, that most of my albuminuric patients yielded sphygmograms indicative of no rise of tension whatever. Nephritis pronounced, with anuria, or with passage of red blood corpuscles or of hæmoglobin in any quantity into the urine, was not—indeed it generally is not—of common occurrence before the end of the second week: and when it was pronounced at about that time it was commonly associated with pyrexia and acceleration of the pulse. I had, it is true, some early cases of hæmorrhagic nephritis, as early

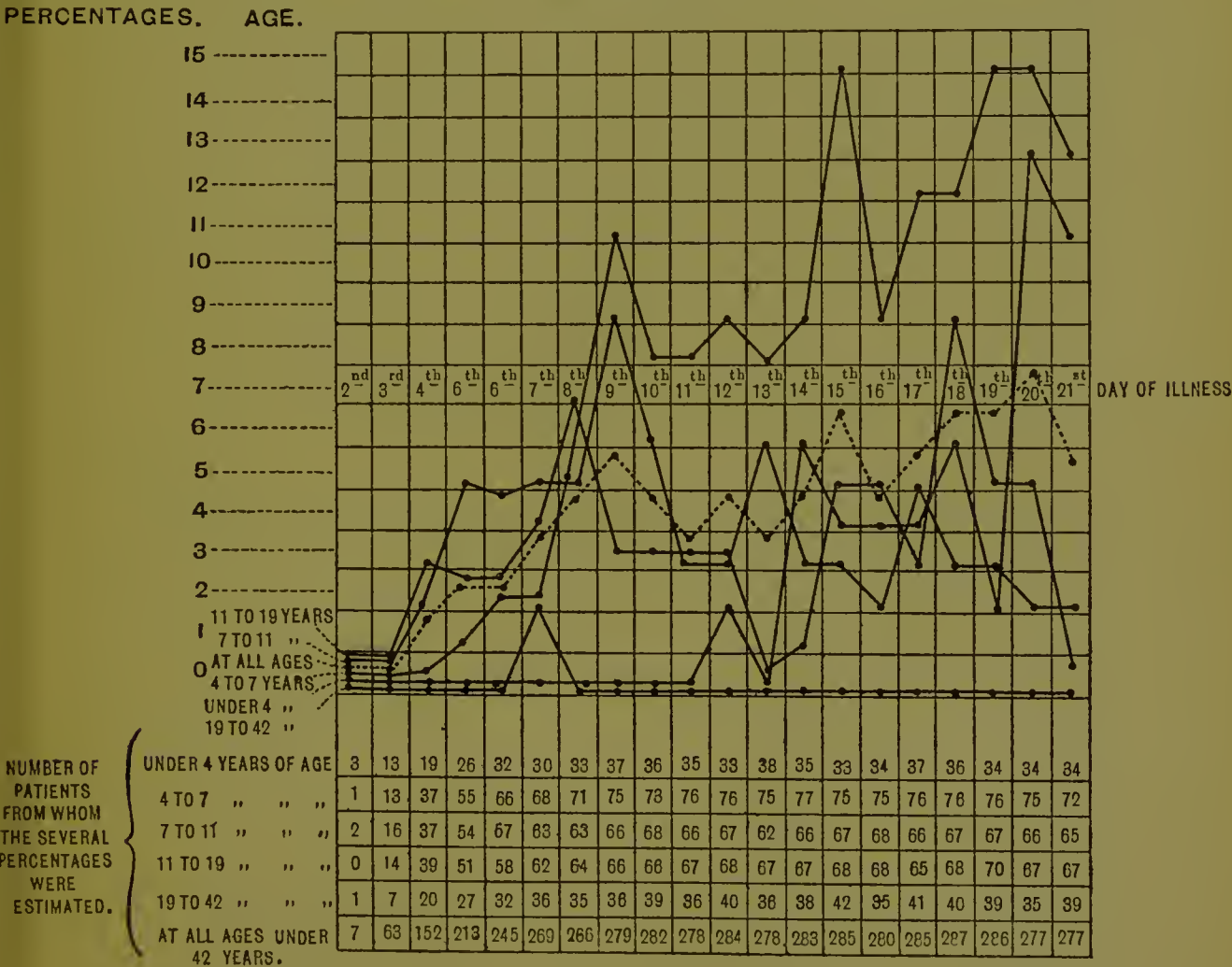
as the third and fourth days of illness; but in each there were pyrexia of some duration, and acceleration, though with slight additional persistency, of the pulse. Then, what signification is to be attached to the slowness when independent, as it was in my cases, of arterial tension? It may be asked whether the slowness were owing to direct action of the virus or of its products on the heart or its nervous mechanism, to simple reaction after previous undue acceleration, or possibly to secondary influence upon the heart resulting from lesser degrees of renal or other lesion. It was a fact that the pulse when slow in the second or third week had generally been greatly accelerated in the first; but in some instances this was not so. There are persons who generally have a slow rate of pulse; but the slowing occurred in so many of my patients, and at so many different ages, that it cannot be explained on such grounds as those herein suggested. The greater proportional frequency of slow pulse among the October and November, as compared with the December and January, patients, may quite possibly have been related, in the matter of its causation, to the wider generalization and more pronounced character of the determinations in the earlier months; and I have been inclined to regard it as due to direct action of the virus upon the cardiac mechanism. Again, *irregularity* of pulse also manifested variations in accordance with age, sex, and season, and, as may be expected, with stage of illness. Facts relating to irregularity of pulse are presented in Table J, and Chart IV. During the first three days of illness there was no irregularity in any case. Taking all the patients together, irregularity was relatively most common (*vide* Chart IV) on the twentieth day; it was somewhat less common on the fifteenth, and slightly less so again on the ninth day. In these respects both sexes and members of each age-group from 4 to 19 years behaved much alike. At ages between 19 and 42 years there was, however, but one person who had an irregular pulse; and at ages under 4 years there was not one with irregular pulse before the twelfth day of illness. The irregular pulse was, I may remark, but occasionally a slow pulse. It was relatively slightly more common among the males, having occurred in 25 per cent. of the males as compared with 22 per cent. of the females. As regards season, it appears (*vide* Table J) that, if persons under 4 years of age be excluded, the irregularity was relatively most common among

the October and November cases in each age-group, and, with only one exception, in both sexes.

TABLE J.—*Showing percentages of patients (among those admitted before the ninth day of illness) who presented irregularity of pulse at some time or other during the first 21 days of illness.*

		Oct. and Nov.		Dec. and Jan.	
Under 4 years of age	males	3 in 13, i. e.	23.0°/o	3 in 9, i. e.	33.3°/o
	females	1 in 8, i. e.	12.5°/o	4 in 9, i. e.	44.4°/o
4 to 7 years	males	13 in 33, i. e.	39.3°/o	0 in 7, i. e.	0.0°/o
	females	6 in 20, i. e.	30.0°/o	2 in 12, i. e.	16.6°/o
7 to 11 years	males	6 in 25, i. e.	24.0°/o	0 in 10, i. e.	0.0°/o
	females	4 in 25, i. e.	16.0°/o	2 in 13, i. e.	15.3°/o
11 to 19 years	males	5 in 22, i. e.	22.7°/o	0 in 10, i. e.	0.0°/o
	females	5 in 25, i. e.	20.0°/o	2 in 13, i. e.	15.3°/o
19 to 42 years	males	1 in 18, i. e.	5.5°/o	0 in 12, i. e.	0.0°/o
	females	0 in 14, i. e.	0.0°/o	0 in 6, i. e.	0.0°/o

CHART IV.—Showing, for successive days of illness, the percentages of patients who presented irregularity of pulse.



It may be asked whether stimulants or oxygen had any influence

in bringing about the above results. Doubtless they had some ; but not, I think, sufficient to affect materially the percentage statistics that have been given.

It may here be mentioned that in the *pulse-curves for individuals* there were three stages (seen also, though obscurely, in the average pulse-curves for aggregates of individuals, given in Chart III) in the course of the illness, when variation in the fall of pulse-rate was most frequent, viz. on the fifth, sixth, and seventh days, on the tenth and thirteenth, and again on the seventeenth and eighteenth days. Variation at these stages occurred with each age-group. And it deserves notice that the three stages, when the pulse was most frequently *irregular*, followed closely upon the stages, when the pulse-rates showed special tendency to variation. These variations will be further considered later on.

CHAPTER V.

CONDITIONS OF TONGUE.

VARIOUS conditions of tongue are met with in Scarlatina. There is in the first place, and perhaps most commonly, the so-called scarlatinal tongue, a condition often spoken of as pathognomonic of the disease, but one which occurs in some other affections also. The chief feature of the *scarlatinal tongue* appears very early in the illness as a thick white furred coating dotted with turgid red papillae: the fur falls away, commencing at the tip and edges, sometimes in flakes, sometimes in piecemeal fashion, in the latter case leaving a shelving edge to the fur that remains; and finally the whole of the dorsal surface becomes red, clean, moist, and prominently papillated. In fact the superficial parts of the tongue, which are of epiblastic origin, are modified in much the same way as the skin; and, being moist, the superficial epithelium is quickly shed. It is needless to say that in mild, or very mild, cases the tongue may be scarcely altered from the normal, presenting perhaps no more than a thin white red-dotted fur, which persists but a short time.

Besides these, there are other conditions of tongue which bespeak gravity of attack. One, very rare, I have called the lichenoid. Others are the ulcerated and the dry. In the latter form the tongue may be cracked, or even balled. In some cases the dryness does not affect the whole of the dorsum of the tongue, but only the centre; and then three or five very readily distinguishable areas are commonly presented. I had 36 cases in which the latter condition of tongue occurred. I have called it the triareal or pentareal dry tongue. In the triareal tongue there are three areas: viz. (1) a central longitudinal strip which is dry; (2) a peripheral area including the edges and tip, in all a horseshoe-shaped area, which is moist, red, and clean; and between these (3) another horseshoe-shaped area, which, by reason of its relation to the central area, I

have called the paracentral area or the paracentre, and which generally presents a white, moist, and red-papillated fur. In progress towards recovery, the peripheral area widens at the expense of the paracentre, and the latter extends itself at the expense of the central area. Should the patient become worse, the central area lengthens even to the tip: it thus cuts apart the limbs of the horseshoe-shaped areas, and the triareal tongue becomes pentareal; at the same time it generally encroaches outwards on the paracentre, and the latter in turn on the peripheral area.

Among the various features presented by the tongue in disease Dr. Dickinson speaks of one in which the tongue, after being coated or furred, or encrusted, becomes flayed down a central strip; and he says that in these cases the central strip is often fringed with a white fur or coating as a line down each side of it, outside which fur there is the nearly normal lateral margin. This condition of tongue would agree with the one, which I have spoken of as the triareal, were it not that the tongues referred to by Dr. Dickinson had already gone through a prior process of encrustation, of furring, or of coating; whereas those, to which I refer, may be seen very early in the course of the disease, and the tongue may be almost normal the day before the triareal condition is manifested. What the histological nature of the central strip in my cases may have been I do not pretend to say; evidently it cannot have been the result of a flaying process. I should add, perhaps, that the condition here referred to may be preceded by a stage in which the only abnormal appearances are the paracentral white fur and a very slight furring of the central area, so that there is also a moist triareal condition; and this may disappear or may pass on into the dry triareal condition. The paracentral fur at times is unilateral, but only rarely so.

These graver conditions of tongue are classed in Table K, from which it will be observed that gravity of attack, in so far as it was revealed by condition of tongue, was most frequent in November, and very much rarer in December and January. I am not aware that stimulants or oxygen had any share in bringing about these results.

TABLE K.—*Showing numbers and percentages of patients who presented graver conditions of tongue.*

	Ulcerated.	Dry		Lichenoid.	Totals.	Percentages proportional to total monthly numbers of patients.
		All over dorsum.	Over part of dorsum (triareal or pentareal).			
October patients	5	20	21	0	46	26.1
November „	4	6	12	1	23	29.4
December „	3	8	2	0	13	16.4
January „	1	3	1	0	5	11.1

CHAPTER VI.

ARTHRITIS.

IN attempting to deal statistically with so-called scarlatinal rheumatism, I shall take up arthritis alone; because inclusion of other conditions, which have been classed, whether rightly or not, under the term rheumatism, might involve confusion: some would extend the term, and I think with right, to certain cases of pericarditis and endocarditis; and, I doubt not, it should include some forms of scarlatinal headache, as Dr. Lauder Brunton has suggested, and as was not inconsistent with the results, which I obtained in 1881 and 1882 by treating scarlatinal patients with salicylate of sodium. It is to be noted that those patients who had previously suffered from rheumatic fever were especially liable to manifest arthritis, and that in them this affection was generally more lasting than in those who had not been thus previously affected: I shall, therefore, deal chiefly with the latter class.

Of the former there were among my patients in all 5, of the latter 28.

Seeing that my patients were not consecutively admitted, and that I have obtained full particulars in regard of arthritis among the 64 intermediate patients; I shall give statistics for arthritis for the whole series, in all 652. These are classified in Table L.

The several influences of age, sex, and season are rendered apparent in this table.

In respect of age, the absence of arthritis among all of the patients under three years of age is to be noted, i.e. in 34 males and 27 females; 50 of these were attacked in September, October, and November, months in which liability to arthritis was, as has already been said, most marked: it will be observed also that at ages over ten years males had the affection more than twice, and females more than four times, as often as at ages between three and ten years.

In respect of sex, it appears that (setting aside those who had previously had arthritis, and those under three years of age) 6·4 per cent. of the females suffered from arthritis, and only 3·2 per cent. of the males; there was relatively a slightly larger incidence upon the females than the males under ten years of age, and a considerably larger incidence on females at ages above ten years. Again, of the September patients, the 10 females who had arthritis, had it collectively, without treatment, for 44 days, an average for each of $4\frac{2}{5}$ days; whereas the 6 males attacked in the same month and similarly affected (with arthritis) manifested the affection, without treatment, for only 19 days, i.e. an average of $3\frac{1}{3}$ days. Further, it may be noted that at ages from ten to fifteen years there were attacked in successive months, commencing with September, of males 11, 18, 7, 5, and 1, and of females 22, 18, 14, 8, and 9; and that while among the males there was no arthritis, among the females there were 4 cases.

These statistics regarding age and sex have a further interest when taken in conjunction with those concerning rheumatism occurring independently of scarlatinal infection, recently adduced by Dr. Cheadle.

Seasonal influence has already been briefly alluded to. Among the 125 November, December, and January patients of ages between three and ten years, there was no evidence of arthritis; and among the 52 December and January patients over ten years of age, there was evidence of it in only 2 cases, and these in the persons of females. Of all these 177 patients there were therefore but 2 affected with arthritis, i.e. 1·1 per cent. On the other hand, among the 409 earlier patients as many as 27, i.e. 6·6 per cent., manifested this affection. Again, whereas upper and lower limbs were affected in each of 7 patients attacked in September and October; the affection was confined, in those attacked in November, to either the arms or the legs; and finally, in those, who fell ill in December and January, wholly to the arms. The affection, moreover, continued without treatment for a longer period than three days in as many as 15 out of the 23 September and October cases; whereas it continued this length of time in only 1 of the 6 November, December, and January cases. In the January case, it continued for only a portion of one day. It may also be noted that it commenced on or after the eighth day in 12 of the 22

TABLE I.—Numbers of persons affected with arthritis among those of 652 consecutive scarlatinal patients

		<i>who had not been</i>									
		No. of patients affected ; total no. of patients of corresponding									
		Over 3 and under 42 years of age.		Over 3 and under 10 years of age.		Over 10 and under 42 years of age.					
	Under 3 years of age.	Total.		Males.		Females.		Males.		Females.	
		Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.	Males.	Females.
September cases	0 in 21	16	10	3 in 73, i.e. 4.1°/o	2 in 50, i.e. 4.0°/o	3 in 26, i.e. 11.5°/o	8 in 35, i.e. 22.8°/o	3 in 36, i.e. 0.0°/o	0 in 36, i.e. 0.0°/o	3 in 42, i.e. 7.1°/o	
October	0 in 18	7	6	4 in 49, i.e. 2.0°/o	3 in 58, i.e. 5.1°/o	0 in 24, i.e. 0.0°/o	2 in 24, i.e. 8.3°/o	0 in 24, i.e. 0.0°/o	0 in 24, i.e. 0.0°/o		
November	0 in 11	4	2	0 in 23, i.e. 0.0°/o	0 in 24, i.e. 0.0°/o	0 in 28, i.e. 0.0°/o	1 in 13, i.e. 7.6°/o	0 in 28, i.e. 0.0°/o	0 in 28, i.e. 0.0°/o		
December	0 in 3	1	1	0 in 26, i.e. 0.0°/o	0 in 28, i.e. 0.0°/o	0 in 19, i.e. 0.0°/o	1 in 15, i.e. 6.6°/o	0 in 19, i.e. 0.0°/o	0 in 19, i.e. 0.0°/o		
January	0 in 8	1	1	0 in 5, i.e. 0.0°/o	0 in 19, i.e. 0.0°/o	5 in 179, i.e. 2.7°/o	15 in 129, i.e. 11.6°/o				
	0 in 61	29	20	4 in 176, i.e. 2.2°/o	5 in 179, i.e. 2.7°/o						
<i>previously so affected.</i>											
		age and sex ; and percentage proportion of former to latter.									
		Over 10 and under 25 years of age.		Over 25 and under 42 years of age.		No. stimulated with port wine or brandy prior to evidence of arthritis.		No. affected in		Who had been previously so affected.	
		Total.		Males.		Females.		Both extremities, only.		Arms only.	
		Males.	Females.	Males.	Females.	Males.	Females.	Legs only.			
September cases	2 in 18, i.e. 11.1°/o	6 in 32, i.e. 20.0°/o	1 in 34, i.e. 0.0°/o	1 in 8, i.e. 12.5°/o	2 in 3, i.e. 66.6°/o	1	5	9	2	4	
October	0 in 34, i.e. 0.0°/o	1 in 38, i.e. 2.6°/o	0 in 11, i.e. 0.0°/o	0 in 2, i.e. 0.0°/o	2 in 4, i.e. 50.0°/o	0	2	2	3	0	
November	0 in 11, i.e. 0.0°/o	3 in 22, i.e. 13.6°/o	1 in 12, i.e. 8.3°/o	1 in 5, i.e. 20.0°/o	0 in 2, i.e. 0.0°/o	0	0	3	1	0	
December	0 in 16, i.e. 0.0°/o	1 in 12, i.e. 8.3°/o	0 in 1, i.e. 0.0°/o	0 in 6, i.e. 0.0°/o	0 in 1, i.e. 0.0°/o	0	0	1	0	0	
January	0 in 1, i.e. 0.0°/o	1 in 14, i.e. 7.6°/o	0 in 1, i.e. 0.0°/o	0 in 1, i.e. 0.0°/o	0 in 1, i.e. 0.0°/o	0	0	1	0	1	
	2 in 80, i.e. 2.5°/o	12 in 118, i.e. 10.1°/o	2 in 22, i.e. 9.0°/o	4 in 11, i.e. 35.4°/o		1	7	16	6	5	

September and October cases ; whereas it commenced thus late in only 1 out of the 6 November, December, and January cases. Arthritis was, it seems, of good augury, and the more so the earlier it appeared.

[If pericarditis without joint affection be classed along with arthritis, it will be necessary to include the case of Archibald G., aged twenty-three years, who fell ill October 27th. In the other patients, tabulated above, there was no discoverable cardiac mischief, i.e. among those who had not had rheumatic manifestations previously to the scarlatinal attack.]

The table shows also the distribution of the affection in the joints ; the upper extremities having been affected in 23 patients, and the lower in only 13. The table shows too that stimulants had no influence in bringing about the determinations in and about the joints ; for prior to the manifestation of the affection, only one patient had had stimulants : and, as possibly bearing upon the duration of the affection, it may be added that none of the patients took stimulants while the affection continued. Oxygen was administered to none of the arthritic patients. Whether its administration to others prevented the appearance of arthritis it is impossible to say ; and I accordingly append the age and sex in each case in which it was used, viz. males of ages 15, 19, 19, 4, 7, 23, 10, 5, 23, and females of ages 2, 9, and 11 years.

CHAPTER VII.

CORYZA, TYMPANITIS, DESQUAMATION, INCIDENTAL RASHES, AND DIARRHOEA.

CORYZA and tympanitis (or *otitis media*) in Scarlatina are of special frequency in early life. Among my patients coryza was relatively rather less frequent among the December cases, and still less among the January cases; and it occurred only in the young. Tympanitis¹ also revealed, as shown in Tables E and M, the influence of season and age. As regards age, it will be seen that by far the larger proportion of cases of tympanitis occurred among patients under eight years of age in each month, except November; there was not one instance of it in patients over twenty-nine years of age. Its incidence was of greater relative frequency on the males among the September and October cases: and though this relation was reversed among the patients attacked later, it must be observed that the numbers dealt with after October were but small. The influence of season is evidenced in the fact that whereas the percentage of cases suffering from tympanitis among the September, October, and November patients was at or over 8.2, in December it fell to 6.3. More detailed enquiry into the influence of season is rendered difficult by reason of the small number of patients dealt with.

¹ Tympanitis in Scarlatina is generally attributed to direct extension of inflammation from the throat, and its frequency in scarlatinal children to frequency of throat trouble in them. In 'Some Pathological Bearings of Darwinism,' published in 1886, I ventured to suggest that it was a primary affection, at least as primary as tonsillitis itself or the rash. It may be borne in mind that the epithelium of the mucous membrane of the tympano-eustachian cleft, homologous with that of the permanently open 'spiracle' of the Selachii, is epiblastic, as also is that of the nasal cavities: and the differentiation of these mucous membranes from the skin proper may be supposed to be less complete in the infant and child than in the adult, so that affections of the middle ear and nose may be looked for more frequently among the former: in other words, if one part of the epiblast be affected we might expect in other parts of the epiblast a greater liability to the same affection in early than in late life.

Desquamation of the epidermis varies¹ immensely in Scarlatina. Scarlatina may unquestionably occur without any or with but

TABLE M.—Numbers of patients affected with Tympanitis.

	Total.	No. affected in		Under 8 years of age.		Over 8 and under 29 years of age.	
				Males.	Females.	Males.	Females.
		One ear only.	Both ears.				
September cases	17	12	5	11 in 66, i. e. 16.6%	5 in 59, i. e. 10.0%	1 in 42, i. e. 2.3%	0 in 43, i. e. 0.0%
October	19	16	3	13 in 52, i. e. 25.0%	3 in 43, i. e. 6.9%	2 in 39, i. e. 5.1%	1 in 40, i. e. 2.5%
November	7	4	3	1 in 24, i. e. 4.1%	1 in 11, i. e. 9.0%	2 in 18, i. e. 11.1%	3 in 19, i. e. 15.7%
December	4	3	1	2 in 22, i. e. 9.0%	2 in 21, i. e. 9.5%	0 in 23, i. e. 0.0%	0 in 9, i. e. 0.0%
January	3	3	0	1 in 9, i. e. 11.1%	2 in 17, i. e. 11.7%	0 in 1, i. e. 0.0%	0 in 25, i. e. 0.0%

¹ Prof. Thomas said: ‘ Diese Abschuppung kann sich nach Zeiteintritt und Dauer, Intensität und Form in verschiedener Weise verhalten.’

the very slightest peeling; such cases occur perhaps most frequently in babes and younger infants. Peeling is commonly more marked in the dark- than the light-complexioned: in the very fair it may be but the shedding of the finest silvery scales, and yet be for the individual marked peeling. When the skin is very moist, the epidermis may be shed in a macerated condition, and so finely broken up that it is easily overlooked. Having in mind these and other possibilities, I took note of the amount of peeling in a large number of cases (cases in no way whatever selected), and I recorded

TABLE N.—*Desquamation among patients over three years of age.*

	Among all the patients examined.		
	No. examined.	Very light.	Very marked.
October cases	121	47, i. e. 38.8°/o	18, i. e. 14.8°/o
November „	55	20, i. e. 36.3°/o	9, i. e. 16.3°/o
December „	66	32, i. e. 48.4°/o	7, i. e. 10.6°/o
January „	31	14, i. e. 45.1°/o	3, i. e. 9.6°/o
	Over 3 and under 10 years of age.		
	No. examined.	Very light.	Very marked.
October cases	66	26, i. e. 39.3°/o	11, i. e. 16.6°/o
November „	29	13, i. e. 44.8°/o	4, i. e. 13.7°/o
December „	39	14, i. e. 35.8°/o	5, i. e. 12.8°/o
January „	18	11, i. e. 61.1°/o	1, i. e. 5.5°/o
	Over 10 and under 42 years of age.		
	No. examined.	Very light.	Very marked.
October cases	55	21, i. e. 38.1°/o	7, i. e. 12.7°/o
November „	26	7, i. e. 26.9°/o	5, i. e. 19.2°/o
December „	27	18, i. e. 66.6°/o	2, i. e. 7.4°/o
January „	13	3, i. e. 23.0°/o	2, i. e. 15.3°/o

it as of different degrees, 1, 1½, 2, 2½, 3, the latter number representing the most marked peeling. Turning to my notes it appears that if patients under three years of age be set aside, their peeling having generally been so very slight, and if degrees 1 and 1½ be spoken of as very mild and degree 3 as very marked, the facts may be put together as in Table N.

Marked peeling was on the whole relatively most frequent in November. In December there were suddenly both relative increase of cases presenting light peeling and relative decrease of

cases presenting marked peeling. I would repeat that these cases were in no way whatever selected; the number of cases examined having been determined solely by the time at my disposal. It needs to be borne in mind, however, that marked peeling does not necessarily signify severity of attack; but certainly there is a general relation of this sort. Prof. Thomas, speaking on the subject of desquamation in relation to character of rash, said: 'Die Intensität des Abschuppung ist in gewisser Hinsicht vom Exanthem abhängig; man darf aber nicht annehmen, dass jedesmal auf ein intensives Exanthem eine reichliche Absehung, und umgekehrt auf ein schwaches Exanthem eine geringe Absehung folgen müsse.' To this, for my part, I would add that, speaking generally, a very marked rash signifies severity of attack; and that stress is also and more especially to be laid upon the earliness of the peeling as an indication of severity, the peeling in really severe cases being indeed general over the whole body at a very early stage—so early in some instances as the second day.

Incidental rashes, of one sort and another, to the exclusion of such as are contagious or infectious, were presented by not a few of the patients under consideration; they were, for instance, herpes, psoriasis, urticaria, and erythematous, papular, and pustular rashes. In a few cases, intercurrent mischief was evidently the determining cause; in others, the rashes may have been such as would have developed in any case, scarlatinal infection wholly aside. Nevertheless, it is of interest to remark that the percentage of patients who presented rashes, whether of one kind or another, was larger among those attacked in September and October than it was among those attacked later. And this time-distribution of the rashes should, perhaps, be taken into account in considering the influence of season. As for age, it appears that no patient under two years, or over twenty years, presented any other than the scarlatinal rash. Among patients at intermediate ages, the rashes were fairly equally distributed. For, of the 36 patients, in all affected, 23 were over two and under nine years of age, and the remaining 13 were over nine and under twenty years of age; thus bearing to the total numbers of patients, at corresponding ages, proportions in the first set of cases of 7.0 per cent., and in the second set of 7.2 per cent. The percentage, however, at three years of age was 12.0, and at six years it was 8.3. In regard to

TABLE O.—*Incidental rashes, not infectious nor contagious.*

No. of patients who exhibited rashes ; no. of patients of corresponding age and sex ; and proportion of former to latter.													
		Over 2 and under 9 years.				Over 9 and under 20 years.							
		Under 2 and at and over 20 years.		Males.		Females.		Males.		Females.			
September cases	0	15 in 183, i.e. 8.1%	2 in 72, i.e. 2.7%	9 in 53, i.e. 16.9%	1 in 23, i.e. 4.3%	3 in 35, i.e. 8.5%							
October	0	14 in 153, i.e. 9.1%	5 in 50, i.e. 10.0%	4 in 46, i.e. 8.6%	4 in 31, i.e. 12.9%	1 in 26, i.e. 3.8%							
November	0	3 in 63, i.e. 4.7%	0 in 24, i.e. 0.0%	0 in 9, i.e. 0.0%	1 in 14, i.e. 7.1%	2 in 16, i.e. 12.5%							
December	0	3 in 65, i.e. 4.6%	0 in 24, i.e. 0.0%	2 in 21, i.e. 9.5%	0 in 14, i.e. 0.0%	1 in 6, i.e. 16.6%							
January	0	1 in 40, i.e. 2.5%	1 in 8, i.e. 12.5%	0 in 18, i.e. 0.0%	0 in 1, i.e. 0.0%	0 in 13, i.e. 0.0%							
The more common of the rashes, and the no. of patients affected with them.													
		Stimulated with port wine or brandy prior to development of rash.						No. of patients who manifested rashes and also					
								Albuminuria					
		Herpes.	Psoriasis.	Urticaria.	Papules.	Pustules.	Erythema.	Diarrhoea.	Mild.	Moderate.	Severe.	None.	Not examined.
September cases	2	1		1	3	2	2	2	?	?	?	?	?
October	1	0		1	2	4	2	2	5	4	4	—	1
November	2	0		0	0	0	0	0	1	2	—	—	—
December	0	1		1	1	0	0	0	2	1	—	—	—
January	0	0		0	1	0	0	1	—	—	—	—	1

sex, the distribution of these rashes varied from one time to another, the rashes having been in excess among the males at one time and among the females at another. Only a few of the patients thus affected had taken port wine or brandy prior to the appearance of the rash; and only a few suffered from diarrhoea. Albumen was found in the urine of all but two of these patients; and in the case of one of the two, no opportunity for examination of the urine was obtained.

Concerning *early diarrhoea*, I would draw attention to the influence of season, as shown by the figures in Table E and Chart II. It occurred for the most part among the younger patients. The precise signification of this diarrhoea may be questioned. I do not think it is attributable by any means wholly to intestinal lesion; putting aside diarrhoeas associated with nephritis, there are others which seem to be associated with resolution of inflammatory exudations, and others which may, doubtless, be due to interstitial hepatitis.

CHAPTER VIII.

FATALITY ; AND THE POSSIBLE INFLUENCE OF A CERTAIN UNAVOIDABLE SELECTION OF CASES ON THE STATISTICAL RESULTS.

THE death-rate among my cases was 7·6 per cent. As shown, however, in Tables E and P the rate was not the same for the different months : it increased from September to November, and it fell markedly in December. Several factors may be suggested as having had causal relation to these variations of fatality. First may be considered age. Of the total 45 deaths, 40 occurred in persons under ten years of age ; and the proportion of deaths

TABLE P.—*Fatality.*

	Deaths.							
	Within 48 hours of ad- mission.	Among those admitted		Among		Total.		Per- centages among patients under 10 years of age.
		On or before the 5th day of illness.	From Wands- worth, Clapham, or Lambeth.	Males.	Fe- males.	Numbers.	Per- centages.	
September cases	4	4	1	7	8	15	7·3	9·7
October ,,	5	11	9	6	9	15	8·5	11·0
November ,,	1	6	5	3	4	7	8·9	15·9
December ,,	0	4	4	2	2	4	5·0	6·1
January ,,	0	4	4	3	1	4	8·8	13·7

per 100 patients under this age increased through September, October, and November, and fell largely in December. [There were 2 deaths among infants under 1 year of age, one attacked in November and one in January.] Secondly, there is sex. As shown in Table P, there were 21 deaths among males and 24 among females ; and this though the males exceeded the females in numbers at most ages. Whether this difference obtained throughout the epidemic I am not able to say. The figures given

in the table regarding stage of illness on admission, show that most of the patients who died, with the exception of those attacked in September and October, were admitted on or before the fifth day of illness. The use of stimulants was, as appears from Table E, somewhat unequal among the patients attacked in the different months; but in this, as in other points, the November and December patients were treated very much alike. The administration of oxygen did, however, in my opinion, modify the monthly mortality, as stated in the paper in *The Practitioner* already referred to. If I am correct in my estimate, there expressed, of its influence, it will be necessary to allow for at least two patients, one attacked in October and the other in November, who were, I thought, saved by its use; and, this being so, the fall in December would be more pronounced than the figures of Table P represent.

There are also yet other considerations to be borne in mind in estimating the significance of the statistics which I have adduced.

In the first place, my cases were not consecutively admitted; and it becomes of importance to take note, in so far as it may be possible, of the clinical features of the intermediate cases. The South Western Fever Asylum comprises two hospitals (the 'upper' and the 'lower'), entirely separated the one from the other, each with an independent system of administration. The patients sent to this establishment were allotted to either the 'upper' or the 'lower' hospital, chiefly with the view of excluding males over fifteen years and females under five years from the 'lower'; this 'lower' hospital received, in the period September, 1887, to February, 1888, altogether 400 patients. Further, of the patients allotted to the 'upper' hospital some, in all 64 (59 of them females), were selected, as being unlikely to become noisy or unmanageable, for treatment in a temporary wooden hut. I did not have direct charge of any of these intermediate cases; and I have not included any of them, except the 64 when discussing the incidence of arthritis, among the statistics already¹ given. Hence, of the

¹ It should also be stated that I have not included among my cases five patients (members of one family) under my care who were suffering from enteric and scarlet fever, or from a form of scarlet fever curiously resembling enteric fever. Of their clinical manifestations I may name punctate rash, tonsillitis, desquamation, wrist rheumatism, acute nephritis, broncho-pneumonia, ulceration and perforation of small intestines.

intermediate patients there were in all 464, of whom 400 were selected solely on grounds of age and sex, females under 5 years and males over 15 years of age having been for the most part excluded; and 64 of whom were selected as being females, or in a few cases boys, and as showing less likelihood than the others of becoming unmanageable. All of these 64 patients were under 24 years of age, 44 were under 11 years of age; 59 of them were females, 5 were boys. The dates of attack are seen in Table Q.

Hence, concerning the intermediate cases, interest attaches especially to these 64 which were treated in the huts. Inclusion of them with my own cases serves to equalize more nearly the proportions borne by the numbers of patients, who were drawn from Lambeth, Wandsworth, and Clapham, to the total numbers of patients who were attacked in the several months; the percentages of patients admitted from these districts thus becoming for October 57, for November 89, for December 95, for January 71: and, again including the hut-cases, the percentages of patients admitted on or before the fourth day of illness become for October 71, for November 77, for December 63, and for January 89. It must be observed that as there were but 5 males among the hut-patients (2 attacked in October, 2 in November, and 1 in December), the statistics, which have already been given concerning male patients, cannot have been materially affected by so small an omission; and it may at once be said that all of the five suffered from albuminuria, and all, save the December patient, had stimulants.

Let us now turn to the notes recorded of these 64 patients. The tongue was either dry or ulcerated in 10 of them, yielding the following percentages for the patients attacked in the successive months, viz. 25.9, 10.0, 7.6, and 0.0. Stimulants were given to 9 before the fifteenth day of illness (5 of the October and 4 of the November patients); the percentages being therefore for successive months 14.8, 5.0, 0.0, and 0.0. Swellings in the neck or about the ears were recorded in four instances; and the percentages for successive months were 14.8, 20.0, 23.0, and 0.0. The condition of desquamation was recorded for 68.7 per cent. of the whole number; and the proportions of patients for whom it was returned as copious per hundred of those for whom any return was made, were for the successive monthly sets 63.1, 57.1, 66.6, and 50.0.

Arthritis occurred in only one patient, and attack in this case took place in October.

It, therefore, appears that the clinical features of the hut-cases did not widely differ as regards their seasonal distribution from those which have been detailed for my own cases, the patients attacked in October and November appearing to have suffered more than those attacked subsequently, though those who were attacked in October suffered more than those who were attacked in November; i. e. assuming that the percentages of patients who presented a dry or an ulcerated tongue, and of those who were stimulated, were in the case of the hut-patients the best indices of severity recorded in the notes. This conclusion is reinforced, also, by the facts as to fatality. In Table Q. are set out the numbers of patients attacked

TABLE Q.—*Fatality.*

		My cases.		My cases+the hut-cases.		My cases+the hut-cases+the 'lower' hospital cases.	
		Total cases.	Percentage of deaths.	Total cases.	Percentage of deaths.	Total cases.	Percentage of deaths.
		I.	II.	III.	IV.	V.	VI.
August	cases	5	0.0	5	0.0	7	0.0
September	"	205	7.3	205	7.3	339	6.7
October	"	176	8.5	203	8.3	319	9.4
November	"	78	8.9	98	9.1	155	7.0
December	"	79	5.0	92	4.3	169	5.3
January	"	45	8.8	49	8.1	63	6.3

in the several months together with the death-rates—in columns I. and II. for my own cases, in columns III. and IV. for my own together with the hut-cases, and in columns V. and VI. for my own, the hut-cases and those which were treated in the 'lower' hospital; and it will be seen that, if the hut-cases be included with my own, the monthly fatality was greatest in November.

It may, too, be mentioned that, if the 'lower' hospital cases be also included, the fatality was at its height in October, and lowest in December; and that the January fatality was only slightly in excess of that for December.

Secondly, it may be asked whether any selection was exercised as to the class of case, in point of severity, admitted into the Board's hospitals. Were only the severe cases, for instance, ad-

mitted in October and November for lack, say, of beds? There is no evidence to show that selection of the sort here suggested was in operation. The Board have accommodation which is potentially unlimited. There was, till November, a very rapid increase in the number of patients, for whom admission was sought; and the maximum number under treatment on any one day in the course of the epidemic exceeded by four times that of any previous year: but no patient was refused admission; and as the Statistical Committee show, by means of the chart facing p. 10 of their report for 1887, the accommodation was expeditiously extended and just sufficiently to keep pace with this increase. Moreover, the stages of illness on admission, which have just been referred to, would afford no support for the contention that any selection of the above sort took place. The course of the curves of admissions and of deaths in Chart II. may also, perhaps, be appealed to in this connection.

Thirdly, it may be asked (on the part of those who maintain that scarlatina is not so fatal among the well- as among the badly-nourished) whether the abolition of the necessity for obtaining an order from the Relieving Officer had the effect of inducing a progressively larger number of persons of less needy circumstances, and presumably therefore of better average nutrition, to take advantage of the isolation afforded by the Board's hospitals, and thus of reducing the percentage of severe cases among those attacked in the later months. This raises several questions which, however, though of great interest, need not detain us now; for there was no appreciable difference in the class of persons attacked in the several months, or in the condition of their nutrition; and, even if there had been, it would afford no explanation of the relative mildness of the attacks in September.

Finally, the statistics for January must be briefly discussed. Why did the percentages of deaths, and of certain grave conditions, among persons attacked in this month rise above those for December? In the first place, the January cases dealt with were comparatively few. They numbered only 49, even when the hut-cases are included with mine, that is a trifle only above half of any of the previous monthly sets of cases; and, if the 'lower' hospital-cases be also included, they formed even less than a third of any of the previous monthly totals of cases. Secondly, if all

the scarlatinal patients, 1052, treated at the South Western Fever Asylum be taken into account, the rise of fatality for January is only slight; and it is likely enough that, if the clinical features of all the cases were examined and collated, the increase observed in the relative frequency of certain grave conditions in the January cases would be found to be less than that shown in Table E. and Chart II. Hence I would conclude that the January statistics there given are not to be accepted as comparable straightway with those for previous months.

With these statistics aside, those which have been given represent doubtless, as nearly as may be, the natural history of the cases to which they refer. Seeing, moreover, that all the patients in the Board's hospitals were from essentially the same social class; and that they were allotted to the several hospitals on considerations simply of accommodation and proximity; my results may, due regard being given to differences of age, sex, and perhaps locality, be taken to represent the natural history of the hospital-scarlatina among the class of persons from which the Board's hospitals were at the time being filled.

CHAPTER IX.

INFERENCES NOW TO BE DRAWN.

I HAD intended, along with the subjects already dealt with, to speak of the rash (its character, intensity, distribution, duration), of tonsillitis, and of other subjects. I have not, however, opportunity for conveniently taking up these subjects now, but I may say that they point to the same general conclusions as those already drawn. The results obtained in connection with albuminuria, however, still remain to be considered; but, as these are somewhat lengthy, it may be well at once briefly to state the general conclusion to which the data already supplied seem to point. It will be borne in mind that after considering the statistical results, adduced in the foregoing chapters, in their relation to age, sex, and treatment, and after allowing fully for each of these factors, there has been found paramount over all of them the influence of season. Several families contributed multiple cases to the list of my patients; but this does not affect the conclusion here expressed.

The correlation of the various symptoms, and the relation of them to season, seem to show that the virus in October and November reached its maximum capacity for generalizing in the system, and for effecting localized determinations; that it did not at once attain this maximum; and that afterwards, as the disease declined in the community, so the virus lost in the capacity here referred to.

The increase and subsequent decrease of frequency and severity of the symptoms were not, however, uniformly progressive for all of them collectively, some symptoms having attained severally their maxima and minima in these respects before others. Some of them indeed rose again somewhat in January. It would not well accord with what is known of organic phenomena, had the rise been

uniformly progressive or the fall abrupt and permanent ; oscillation in the manifestations at the period of transition would be expected ; and I shall take occasion later on to give further evidence of this oscillation as prevalence and intensity rose and fell away.

I now pass to another scarlatinal manifestation, albuminuria. This will engage attention for some time, the facts, which it will be necessary to introduce in order to carry further the investigation into the influence of season, being numerous and statistical.

SECTION III.

ALBUMINURIA, AMONG THE PATIENTS UNDER OBSERVATION IN ITS RELATION TO SEASON.

SUBSECTION A.

Albuminuria, whether with or without the passage of red blood corpuscles or of hæmoglobin into the urine.

CHAPTER I.

INTRODUCTION.

IN this subsection I shall consider the incidence of albuminuria in its relation to season, whether the albuminuria were or were not associated with the passage of red blood corpuscles or of hæmoglobin into the urine. In subsection B. I shall deal separately with those cases in which albuminuria was thus associated; and in subsection C. with those which did not present either this or any other evidence of renal mischief. It will be necessary, however, before proceeding to the immediate purpose here expressed, to speak of the method employed in testing for albumen.

The urine, passed between 10 a.m. and 12 noon, was examined for albumen, in most cases, regularly every other day; breakfast having been taken by the patients at 8 a.m. The testing was conducted not later than fourteen, commonly not later than ten, hours after the urine had been voided. All practicable care was taken as to the cleanliness of the vessels into which urine was received. A fully saturated watery solution of picric acid, containing not less than 5 grains of the dry acid to the ounce, was the reagent employed; and testing was generally carried on by gas light, a matter of importance if slight opacities are to be detected. It may here be noted that the urine in nephritis at the time of emission

may be alkaline owing to fixed alkali, as was shown by Martin Solon and by Rayer in 1838. The former observer attributed this alkalinity to the salts of soda from the blood-serum. Bartels also spoke of it as occurring persistently in some cases of parenchymatous nephritis. The urine of one of my patients suffering from nephritis continued for several weeks to be passed alkaline during the day-time owing to fixed alkali; and the urine of another albuminuric patient was for some days together alkaline on emission. Attention to the reaction of the urine is of importance when testing with picric acid, the precipitate with alkaline urine being apt to be overlooked. The albumen precipitated, on adding a drop or two of the reagent, at once dissolves on agitation of the mixture; but further addition of the reagent in excess (not less than thrice the volume of the urine may be needed) renders the precipitate permanent.

It is deserving of notice that, on adding picric acid to ammoniacal urine, a very copious lustrous deposit forms, consisting of minute needles, absolutely and altogether different in appearance from any albumen precipitate: the needles, however, (though insoluble in the cold) are readily dissolved on being warmed, when, if excess of the acid has been added, any albumen present remains undissolved. Ammoniacal urine, however, may generally be attributed to uncleanness or some exceptional liability to decomposition, and such urine would not readily be accepted as unaltered renal excretion.

I took picric acid for two reasons; first, owing to the delicacy of its reaction, and therefore its value especially at the commencement and still more perhaps towards the termination of nephritic mischief; secondly, owing to the promptitude with which it can be used. Employment of nitric acid would have involved a much greater expenditure of time; and it was evident that any value, likely to attach to my results, would be the greater the larger the field they covered.

Picric acid is said, however, to precipitate mucin, which, as Berzelius found, is present in minute quantities in normal urine. Dr. Johnson, on the other hand, writes 'it is a well known fact that while picric acid alone gives no opalescence with mucin, acetic and nitric acids, both coagulate mucin'; he also says that nitric acid causes a haziness to appear with mucin. In several instances, to be mentioned below, in which I examined the urine of scarlatinal patients regularly every other day for more than eight weeks, I did not at any time obtain the very slightest turbidity with picric

acid. If it react promptly with mucin my results will be vitiated; for I have called all urines albuminous, which, when perfectly clear to the eye and warm, have promptly yielded a precipitate, on the addition of picric acid in excess. I took care, however, when it appeared to be necessary, to exclude from the urine matters likely to pass out along with it from the genital apparatus or urethra; taking for example the urine passed into a second vessel after a considerable amount had first been passed into another vessel. Such a measure would not, it is needless to say, exclude mucus from the bladder, ureters, the excretory or the secretory parts of the kidney. Still, if mucus were furnished in any quantity from these sources—and I have excluded all finer shades of turbidity from the tables which follow—it would no doubt be pathological; and albumen would doubtless be simultaneously furnished from the same sources: in the next chapter, moreover, I shall give the data which guided me in differentiating the site, from which the albumen was derived.

Peptones are largely, if not wholly, excluded from my records, owing to the fact that the urines were all heated: the same is true of urates, and of alkaloids given (though they were but very rarely given) as medicines.

I made use of various terms for expressing the results of the analyses. *Shade*³ represented a very decided opalescence, due to albumen, which, however, after the urine had stood awhile, did not form more than a thin film of deposit on the inner surface of the test-tube. Lower grades were represented by *shade*² and *shade*¹. A 'trace' was an opalescence of about four times the density of that of *shade*³, a milkiness. Higher grades were variously designated.

CHAPTER II.

GENERAL CONSIDERATIONS AS TO THE ALBUMINURIC INCIDENCE, SOURCE OF THE ALBUMEN.

I HAVE in Section I. spoken in very general terms of the incidence of albuminuria. I would now direct attention to Table R, in which are collated statistics relating to the exhibition of this symptom by the several monthly sets of patients. The record for the period before getting up, presented in this table, shows that whereas all the October and November patients, whose urine was examined (i. e. 92.1 per cent.), had albuminuria: of the December and January patients examined as large a proportion as 13.8 per cent. of the former, and 26.8 per cent. of the latter did not have albuminuria before getting up; and as large a proportion as 31.9 per cent. of the former, and 19.5 per cent. of the latter had it only slightly marked. In fact, among patients who were examined, albuminuria was present in each of those attacked in October and November, and generally (as shown in the table) in considerable degree; whereas it was either wholly absent or present in only the slightest degree in nearly one half (46.0 per cent.) of those attacked in December and January.

The albuminuria before getting up, when present, was constant for a considerable number of days (twelve or more): it was not, as I shall show below, a mere casual incident of pyrexial origin; nor was its decline in the December and January patients to be accounted for by differences as regards sex, age, stage of illness on admission, or treatment.

Further, whereas 29 per cent. of the October and November patients examined had no albuminuria immediately before getting up, only 12 per cent. remained thus free an hour or so after getting up; but whereas 66 per cent. of the December and January patients examined had no albuminuria before getting up, not less than

48 per cent. remained free in this respect an hour or so after getting up. And yet the data governing the getting up of patients were practically the same throughout the period under consideration, so that the facts above stated as to occurrence of albuminuria will serve to reinforce the statement, already made, concerning this symptom in relation to date of attack.

Such are some of the general results obtained in this connection. Further consideration of them I shall for the present defer, to inquire meanwhile into the source of the albumen. First—Was the albuminuria referrible to the kidneys? Secondly—If so, was it due to actual structural change of the renal tissue?

TABLE R.—*Showing numerically the incidence of Albuminuria*¹.

	Admitted.	Allowed to get up when convalescent.	Albuminuria before getting up for the first time.					Albuminuria immediately before getting up for the first time.					Albuminuria an hour or so after getting up for the first time.				
			Not examined.	Examined.	Shade ³ of albumen or more.	Less than shade ³ of albumen.	No albumen.	Not examined.	Examined.	Shade ³ of albumen or more.	Less than shade ³ of albumen.	No albumen.	Not examined.	Examined.	Shade ³ of albumen or more.	Less than shade ³ of albumen.	No albumen.
Oct ^r . cases	176	160	15	161	159	2	0	10	150	55	57	38	12	148	97	42	9
Nov ^r . „	78	67	5	73	67	6	0	2	65	7	34	25	3	64	21	26	17
Dec ^r . „	79	73	7	72	39	23	10	1	72	3	22	47	1	72	16	18	38
Jan ^y . „	45	41	4	41	22	8	11	1	40	2	10	28	0	41	10	14	17

I. Was this albuminuria referrible to the kidneys? Or rather, may the albumen have proceeded from parts of the urinary tract below the kidneys; was the albuminuria, what I may call, transrenal?

It is difficult to find terms severally applicable to the various forms of albuminuria. I shall here use the words (1) transrenal, (2) renal and (3) cisrenal albuminuria to imply the *abnormal* escape of albumen by the urine as a *direct result* first of mischief in parts of the urinary tract below the renal papillae, secondly of mischief in the renal tissues, thirdly of mischief in the blood or of change of blood pressure independent of primary changes in the kidney or in

¹ Statistics for September are not introduced into this table, the pressure of work at the hospital having been too severe in that month to allow of recording facts, negative as well as positive, such as would be of service here. There were, as seen from the table, several patients whose urine was not examined, and several who were not allowed to 'get up'; they were for the most part infants and young children. In only one case was there satisfactory evidence of renal disease prior to the scarlatinal attack. The term 'to get up' is used throughout in its conventional sense.

lower parts of the urinary tract. As examples of *cisrenal* albuminuria I would give paroxysmal hæmoglobinuria; paroxysmal albuminuria; albuminuria resulting from entrance into the blood of a more readily diffusible albumen, such as egg albumen, or of substances such as salts, which render albumen more diffusible; or again, albuminuria due to nervous changes such as that found after epileptic vertigo by Max Huppert.

Renal albuminuria might further be divided into structural and irritative; the latter embracing that large group of cases referred by Dr. Johnson to irritation of the secreting cells of the kidney by matters abnormally present in the blood—owing to chilling of the skin, maldigestion, malassimilation, malmetabolism, hepatic derangement, excessive muscular action, or owing to absorption of them from centres of suppuration and other morbid processes, such as Frank supposes take place in cases of strangulated hernia. Irritative renal albuminuria would verge upon the albuminuria obtained in animals by M. Gaucher by repeated injections of aqueous solutions of xanthin, hypoxanthin, creatin, leucin, and other substances into the system; it passes in fact insensibly into inflammatory albuminuria, one of the forms of structural albuminuria.

It will be observed that I have applied the above terms to the abnormal escape of albumen: the escape of albumen as a normal event, if it do occur as several authors of eminence maintain, being designated physiological albuminuria. I avoid the term functional albuminuria chiefly in consequence of the difficulty of giving to it even a tolerably clear definition.

Put briefly, the albuminuria was, I think, in chief part renal, due to structural change in the kidney, more especially perhaps in the glomeruli; though it would be idle to deny that it may have been in part transrenal.

Most samples of albuminous urine, in fact all passed before the patients were allowed to get up, deposited a sediment containing morphological elements. The sediment thus deposited in urine, passed by patients still in bed, was found in all instances to consist largely of leucocytes; and it became therefore of importance to ascertain, if possible, the source from which these bodies were derived. Did they come from the kidney or from transrenal parts of the urinary tract? Much turns upon the answer to be given to this question.

First, as to the leucocytes themselves. Many of them were free: in most cases they were also found aggregated in large numbers into rounded masses; and, though more rarely, they were to be found, packed closely together in form of a short cast. They varied in shape from ovals to spheres; some were quite clear and highly refractive, others were granular; some were stained in methyl violet very much more readily than others; and, whereas some under the action of this reagent or of acetic acid did not, the majority did, present nuclei—two, three, or four in number. They had the general

appearance of ordinary leucocytes ; many were remarkably like the nuclei of large cells. They, with other morphological elements in the urinary sediments, are figured in the adjoining plate. Secondly, in regard to the origin of these bodies. The researches of Klebs, published in 1869, abundantly confirmed as they have been by later observers, would lend *prima facie* support to the supposition that leucocytes, present as they were in my cases in large numbers, and forming the chief morphological element in every albuminous urine (except perhaps when blood stained) before the patient got up, were derived from the kidneys, probably from the glomeruli ; and this supposition was borne out by the presence, in the same urines, of several bodies, which were unquestionably derived from the renal tubes, such as 'hyaline spherules' and casts. The spherules were present in the urine of every albuminuric patient at one time or another. These bodies were originally described by Oertel ; and they have more recently been examined by MM. Cornil and Brault, who, in confirmation of Oertel's observations, find them due to what they call 'vesicular alteration' of the epithelium of the renal labyrinth : and doubtless the presence of the spherules in my cases indicated the occurrence of this same change in the renal tubules ; perhaps, however, not necessarily in those of the labyrinth, for some have supposed that the spherules may be derived also from the cells of the straight and collecting tubes. The leucocytes were associated also, as I have said, with casts. These casts were in most instances rare, not more than two or three having been found each day after a careful search in the deposit from six ounces of urine. In many instances there was no cast discoverable for several consecutive days, the leucocytes appearing all the same. The casts were of various sizes : some were in part stippled, in part clear ; most of them presented bodies indistinguishable from the free leucocytes in the same urine. These casts were plainly of renal origin : and (seeing that Oertel and others after him have demonstrated the formation of casts by coalescence of hyaline spherules) it is likely that these casts serve, as do the spherules, to indicate structural change in the renal tubules, most probably those of the labyrinths. The leucocytes in the casts had found their way into the tubules either from the glomeruli, or from the walls of the tubules themselves ; and had there become entangled in the substance forming the body of the casts : it is very probable also that

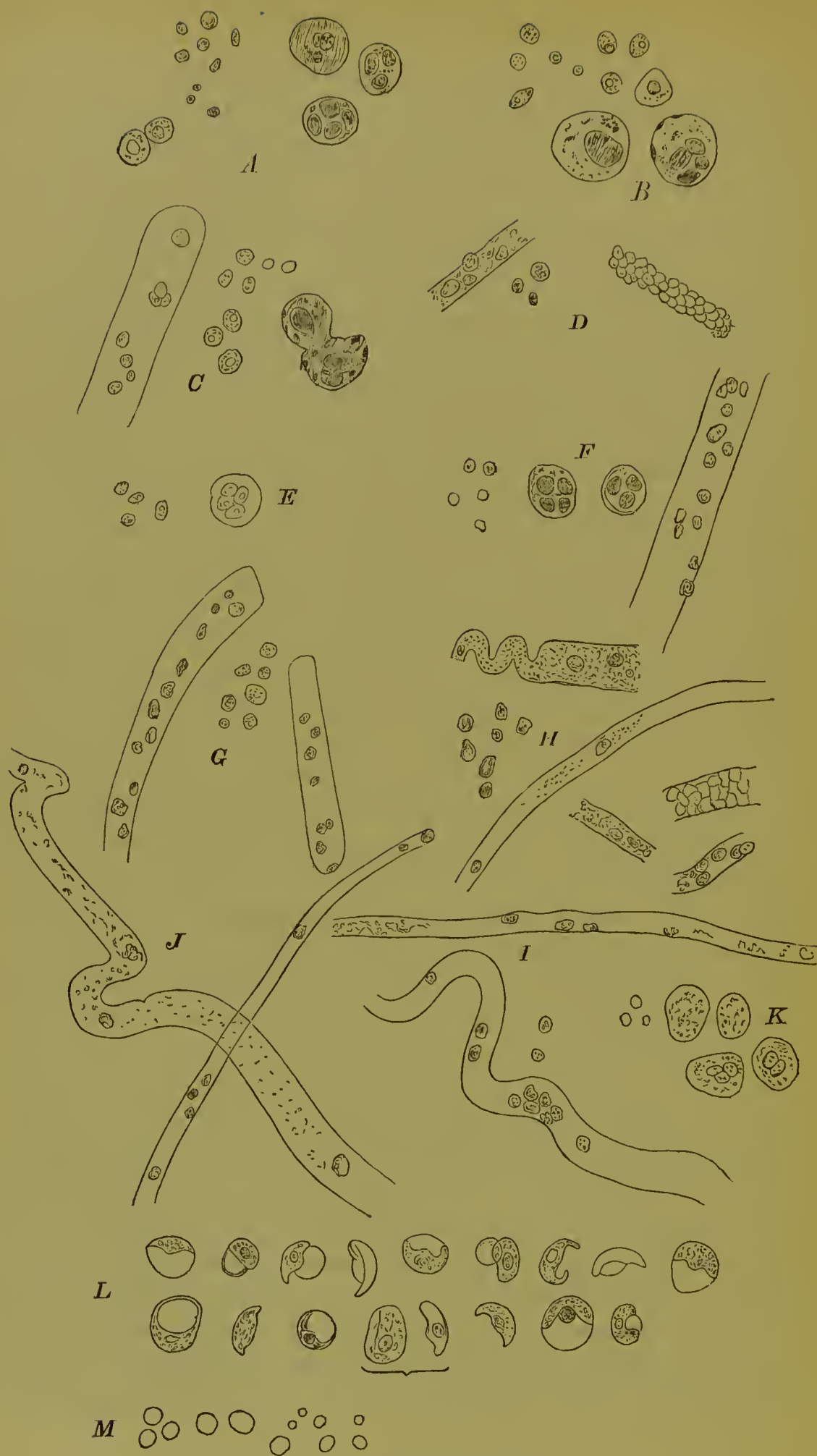


PLATE I.—For description see opposite page.

the leucocytes, which were free in the same urine, had passed down the tubules from a like source. The urinary sediment contained also crescentic cells, the source of which I have failed to ascertain. On consideration of the facts here mentioned I have inferred that the leucocytes proceeded from the renal tubes, probably from their ultimate recesses. There was, however, in the urines of several patients a very considerable amount of epithelium in the form of free cells, derived no doubt from the lower parts of the renal tubules, from the renal pelvis, the ureter, the bladder; and it may be that leucocytes were derived from the same parts: but these epithelial cells were generally found only after the patient had got up; and, moreover, every urine, in which these cells occurred, contained also leucocytal casts, casts indicative in all probability of mischief in the renal labyrinth. It may, moreover, be contended that the urine, when tested, was not absolutely clear, as I have represented it as being; that it contained epithelial and other elements; and that these are answerable for the reaction observed with picric acid. I took some pains to determine this; but I came to the conclusion that cells, such as I met with, even if they were present in the apparently clear urine, would not give the reaction, certainly not promptly. As already said, likelihood of escape of albumen into the urine from the urethra and genital tracts was borne in mind, and, as far as possible, guarded against.

I propose now to review the above statements more closely, and at the same time to present a general historical sketch of matters having relevance thereto.

Previously to the publication of the researches of Professor Klebs

PLATE I.—Showing, at *C* to *M*, specimens of the morphological elements in the sediments of the urine of fourteen patients, at *A* also bronchial sputum, and at *B* nasal mucus. All the figures are from natural objects; they are magnified 160 diameters, except the large corpuscles at *A*, *B*, *C*, *E*, *F*, and *K*, which represent the free leucocytes after being treated with acetic acid or methyl violet and magnified 600 diameters.

The free leucocytes occurred in the same urine as that in which the cast, adjoining them in the plate, was found.

At *K* are leucocytes, two of which when treated with acetic acid did not present nuclei, and two of which did.

At *L* are represented cells, some of which resemble fat- or mucus-cells.

At *M* are the hyaline spherules of various sizes, commonly quite spherical, and always with exceedingly fine single contours.

the significance of leucocytes in the urine, as possibly implying renal mischief, was very generally overlooked, these bodies being assigned by most observers to a transrenal source, to any but a renal source. Purulent matter in bulk in the urine had been allocated to the bladder or kidneys by even the earliest writers. L. Bellini for instance, writing in 1717, said: 'Si quis sanguinem aut pus meiat, renum aut vesicae exulcerationem significat'; but Bellini was concerned with occurrence of leucocytes in mass, in such abundance as to be readily recognised with the naked eye. In more modern times G. Gluge¹, when speaking of an inflammatory form of Bright's disease in the year 1841, described it as presenting two stages, characterized, the first by 'Congestion in den Malpighischen Körpern,' and the second by 'Bildung von Entzündungskugeln in den Malpighischen Körpern und Blutgefässen der Rindersubstanz und Daseyn ausserhalb derselben im Nierengewebe.' Referring to the Malpighian bodies in one case in the second stage, he said: 'Sie verschwanden unter den undurchsichtigen Entzündungskugeln'; a description curiously recalling that by Dr. B. C. Waller (in 1879) of a figure in his paper on glomerulonephritis,—'a Malpighian body entirely hidden by a dense accumulation of small cells.' In another case Gluge described the inflammatory corpuscles as being both inside and outside the vessels and the Malpighian bodies. These were, surely, cases of glomerulonephritis. To have held any definite view as to inflammatory corpuscles passing out by the urine would not have been possible, however, until Bowman in 1842 had given out his discoveries on the structure and relations of the Malpighian bodies to the blood-vessels and tubules. Gluge indeed seems, as Henle said, to have mistaken some of the tubules for blood-vessels. Then Scherer, in 1843, spoke of the urinary sediment in Bright's disease as containing a large quantity of small nucleoli (Aggregationen von kleinen Nucleolis), of mucous corpuscles, and of casts filled with small nucleoli, and appearing to be crouplike pseudo-membranes thrown off from the finest renal tubules; and, referring to a case of scarlatina, he said: 'Der ganze Bodensatz des Harns besteht aus einer Menge einzelner Nucleoli, sowie sehr vielen, in Form von länglichen Röhrchen verbundenen Kernchen.—Viele der Nucleoli sich zu sphärischen Kugeln vereinigt fanden, so sind hier die meisten derselben der

¹ 'Abhandlungen zur Physiologie und Pathologie,' p. 130.

Länge nach agglutinirt.' Scherer seemingly attributed all these elements to a renal source. Vogel in 1843 spoke more explicitly concerning the origin of casts and leucocytes in urine. After describing the casts in renal inflammation he said: 'Nemo fere dubitabit, cylindros illos ex fibrina coagulata in tubulis uriniferis formari, eosque, dum coagulantur, epithelii tubulorum frustula amplecti; unde etiam mirum non est, eos, simulac una cum urina e renibus excernuntur, canalium, in quibus nati sunt, formam referre'; and he added: 'Cylindris non raro magna copia corpusculorum puris addita est; quod quidem tum crediderim fieri, cum et calyces renum et ureteres inflammatione correpti fuerint.' Or, shortly put, the above casts were formed in the kidneys and contained particles of epithelium from the tubules, but the pus corpuscles were transrenal. Henle too, who seems to have been first in clearly establishing the identity of casts in the urine with structures¹ which he found after death in the tubes of the kidney, in speaking of leucocytes in the urine of persons suffering from Bright's disease, said: 'Eiterbildung im Innern der Niere ist im vorliegenden Falle nicht eingetreten gewesen, und auch in den Harnkanälchen konnte kein Eiter gebildet worden sein. Die Eiterkügelchen, die dem Urin beigemischt waren, mussten demnach auf der Schleimhaut des Nierenbeckens entstanden sein.' Henle did not find much alteration of the Malpighian corpuscles in Bright's disease: he says they were '*nur mit vielen Elementarkörperchen und mit Fett bedeckt.*' Henle, like Vogel, would not assign to leucocytes a renal origin: he evidently saw glomerulitis, but he attached no importance to it. Franz Simon in 1844 spoke of the urinary sediment as containing mucous corpuscles (some granular, less or more, and all presenting evident nuclei), also round dark granular corpuscles (very like the inflammatory corpuscles described by Gluge), and casts, some clear, some granular, and all of them containing cells and corpuscles like mucous corpuscles, derived from the epithelium of Bellini's tubules. Heller, again, in 1845, always found mucous corpuscles, and, in the congestive stage, inflammatory corpuscles in the urine. Dr. George Johnson in 1852

¹ In the 'Zeitschrift für rationelle Medicin,' 1844, Prof. Henle, referring to casts in the urine of a person who died of Bright's disease in 1842, said: 'Sie sind offenbar identisch mit den kleinen cylindrischen Stückchen, welche während des Lebens im ausgeleerten Urin gefunden worden waren.'

spoke of the *exudation cell cast*: he thought it was associated, however, more especially with acute suppurative (septicaemic or pyaemic) processes in the kidney; and he regarded the leucocytes in the casts as having resulted from metamorphosis of the epithelium of the tubules, for he had found all transitions between renal epithelial cells and pus cells. He did not refer them to the Malpighian bodies; the glomeruli appeared to present small cells on their free surfaces, but on careful examination these cells were found to be blood corpuscles within the lumen of the capillaries. Dr. Hassall in 1864 described and figured bodies found in urinary sediment, which, it appears, were leucocytal casts and free leucocytes. Sir William Jenner also, in 1865, spoke of pus cells in the urine of Bright's disease: he thought they generally came from the renal pelvis; but he said they certainly also might come from the kidney, for he had seen the renal tubules, in the case of a boy, who had died of scarlatinal dropsy, full of pus corpuscles.

When, however, Klebs¹ made known the results of his researches upon the nephritis of scarlatina, the occurrence of leucocytes and leucocytal casts in the urine acquired a new significance. It had previously been supposed that acute and subacute nephritis were essentially parenchymatous, a sort of catarrh (or, as Dr. Johnson said, a desquamative inflammation) of the tubules, to the exclusion of mischief in the Malpighian bodies. Gluge, as I have shown, Beer, Bristowe and Wilks had already found the glomeruli to be the parts specially affected; and Biermer, Coats, Wagner and Cayley had seen the scarlatinal kidney presenting interstitial changes. It was left, however, for Klebs to draw attention to the frequency with which scarlatinal nephritis was essentially a glomerular affection. Klebs found the only noticeable condition in the kidney to be the accumulation of small angular nuclei and a finely granular matter in the Bowman capsules; he attributed these nuclei to proliferation of the connective tissue which Axel Key had found between the coils of the glomeruli: there was no change of the parenchyma, even though the patient had died of anuria

¹ Vide 'Handbuch der pathologischen Anatomie,' Band I, p. 644: 'Als Glomerulonephritis kann man eine Form der interstitiellen Nephritis bezeichnen, bei welcher ausschliesslich das interstitielle Gewebe der Glomeruli betheiligt ist.—Die reinsten Formen einer acuten Glomerulo-nephritis finden sich im Gefolge der Scarlatina.'

with uræmia. Later observers have found this glomerular affection generally present, but not necessarily as the predominating lesion. Kelsch in 1874, like Klebs, found the glomeruli to be the parts first affected; and he referred the changes of the renal epithelium, found later, to diminution of the blood flow to them from the glomeruli, that is, as he said, to anaemic necrosis. Klein in 1877 found the changes to be: first, increase of the nuclei covering the glomeruli (not however in such abundance as Klebs had described), hyaline degeneration of the tunica intima, and multiplication of the nuclei of the tunica media of the minute arteries (especially those afferent to the Malpighian bodies); next, leucocytal infiltration along the course of the vessels and extending towards the surface of the kidney; and, finally, parenchymatous changes, which were quite insignificant before this infiltration had commenced, and the intensity of which depended upon the degree of this infiltration. He found similar changes in the walls of the arteries in the liver and spleen; as, also, he had previously (in 1875) found in the intestines and spleen in cases of enteric fever. He thought the change in the tunica media was a true hypertrophy, and due, like that of the intima, to some irritant in the blood. A. Meyer, of New York, found confirmation of the views expressed by Klebs. Then Bamberger expressed his opinion that parenchymatous nephritis in all cases originated in glomerular hyperplasia. And C. Weigert by 1879 had come to regard the changes of the renal epithelium in acute nephritis, in parenchymatous nephritis, and in renal cirrhosis as secondary to interstitial changes, the latter impeding the glomerular circulation, or causing more direct impediment to the circulation of nutritive fluid around the tubules. Cohnheim also thought the primary change in all forms of nephritis was in the Malpighian bodies, even in the interstitial form. Dr. B. C. Waller, in 1880, corroborated the observations of Klebs: he also found, round the veins and arterioles, the leucocytal infiltration observed by Klein; but he failed to find the vascular changes which Klein had described. Dr. Waller, moreover, found the fibrotic substance of cirrhosis most abundant around the Malpighian capsules, along the path of the vessels of the general stroma, and immediately under the capsule, i. e. in situations precisely those in which he had observed 'abundant accumulation of unmistakeable leucocytes' in glomerulo-nephritis; and, finding that in the latter affection

the leucocytes tended to fibrillate, he concluded¹ that cirrhosis began in much the same way as diffuse nephritis. Then Dr. Johnson in 1882, having found that the renal mischief in scarlatina was not always desquamative tubular nephritis, but often such as Klebs had described, referred the leucocytes of leucocytal casts, which he had frequently observed, to the glomerular inflammation, as Langhans in 1879 seems to have done. In 1884 Professor Greenfield expressed his opinion that in chronic inflammation succeeding acute inflammation of the kidney, the Malpighian bodies were always affected to a very great extent; and that in all the forms of Bright's disease there were extensive changes in the glomeruli. And in the same year MM. Cornil and Brault spoke of all the forms of nephritis, save only those called by them vascular and glandular cirrhosis, as having common origin in changes that are diffuse from the first, all the elements of the kidney being affected from the first,—the changes, however, predominating now in the glomeruli, now in the renal epithelium, and in other cases in the connective tissue. In all cases of diffuse nephritis they found glomerulitis, and in the urine albumen and leucocytes. They always found glomerular changes in cases in which there had been albuminuria², and they incline to the view that the chief condition for albuminuria is change in the glomeruli. Though this be so, it must be borne in mind that, as Senator observes, the interstitial tissue and the epithelium of the tubules almost always become involved, though it generally be after and secondarily to glomerular changes.

¹ MM. Cornil and Brault do not take this view. They find in cirrhosis (the small red granular kidney) that the Malpighian tufts are thickened and replaced by fibroid tissue, even adherent, may be, to the Bowman capsules; but they do not ascribe these changes to inflammation because they fail to find evidence of cellular proliferation. They would regard the cases of interstitial nephritis, which Waller, Lancereaux and others suppose to commence in diffuse inflammation, as not properly belonging to the class of case which they have in mind, and to which they give the name vascular cirrhosis, but as belonging to that class which Bartels spoke of as secondary atrophy. It may, however, here be noted that several authors seem not to recognise less severe forms of diffuse nephritis: they speak of the latter, indeed, as being always a severe affection.

² Vide 'Études sur la Pathologie du Rein, par Cornil et Brault,' p. 69: 'De notre côté, nous sommes arrivés à cette conclusion, que, chaque fois que l'albumine est décélée par les reactifs dans l'urine et que l'examen du rein peut être fait, on trouve toujours des lésions glomérulaires. Il n'est pas nécessaire que la lésion soit très intense, et il semble même que dans certains cas, une simple perturbation fonctionnelle suffise à permettre le passage de l'albumine.'

Weigert, too, finds that, when albuminuria in animals is due either to acid salts of chromium or to petroleum, the epithelium of the convoluted tubes alone is affected. Litten also supports the view that albumen escapes from the tubules. Nevertheless, there is evidence, derived from many sources and supplied by many observers, to show that the glomeruli are commonly the parts of the kidney where inflammation is first manifested, and from which albumen is in large part derived: indeed, it has been said that the glomeruli may be the only parts to show changes of importance in the kidneys of scarlatinal persons who have died of anuria with uraemia. And there is evidence enough that leucocytes are to be found very early in the cavities of the Bowman capsules.

If these facts be considered along with those detailed above concerning my own cases, and with the further fact that there was no marked lesion, in my cases, in the tubules (as may be inferred from the absence of that variety of structure and size in the casts, such as is found in the urine from kidneys, the tubules of which are to any marked extent affected); we may in them see justification for supposing that most, if not all, of my albuminuric patients suffered from glomerular changes, inflammatory in nature. I should, however, add that there was not, in most of my cases, any great constitutional disturbance: and that as the observations, which have been made by authors on glomerulo-nephritis, have for the most part, if not wholly, referred to fatal cases; it may be that milder forms of scarlatinal nephritis have not yet been sufficiently studied to justify any very emphatic assertion concerning their manifestations.

Further towards identifying the seat of mischief within the kidney, the ground-substance of the casts may be considered. Vogel spoke of casts as being formed of fibrin. Henle took the same view. Axel Key then asserted that the chemical reactions of all kinds of casts differed from those of fibrin: he, with Litten, Bizzozero and others, thought that casts were formed by the agglomeration of degenerated renal cells. Oertel, Rovida, Aufrecht, Cornil and Brault have supposed that some casts are formed by fusion of those hyaline spherules, which have been already spoken of as resulting from what the two latter observers call 'vesicular alteration' of the cells of the convoluted tubes. Cornil and Brault suppose that the spherules are one form in which lymph-plasma passes through the cells into the lumen of the tubules; they

suppose, indeed, that this plasma may pass into the tubules as a mere fluid transudation, and then take part, as Klebs, Rindfleisch, Posner and others have thought, in the formation of casts. Dr. Johnson has always maintained that the basis of all casts is fibrin. There can be no dispute as to the likelihood of fibrin entering into the constitution of blood-casts, and possibly of some other forms of cast; but there is strong evidence to show that the body of many casts is formed by the fusing together of the spherules above referred to, and which, though regarded as of proteid composition, are not known to be fibrinous. These spherules were found in the urine of all my albuminuric patients: most of them were perfectly clear, a few were granular; they had no nucleus, and they did not become coloured in staining reagents. Cornil and Brault say they consist of proteid substance; they are apparently not fatty. Oertel, who first spoke of them, supposed that they were formed by, and expressed from, the epithelial cells, and that hyaline casts were formed by their fusion. Rovida confirmed Oertel's observations: Bartels added further confirmation, the figure on p. 77 of his work, entitled, '*Allgemeine Symptomatologie der Nierenkrankheiten*,' being given in illustration. Aufrecht, also Strauss and Germont supported these views. And MM. Cornil and Brault in 1884 showed¹ beyond dispute that these spherules resulted from alteration of the cells of the convoluted tubes; that they were expressed from the cells into the tubules, and that there they became fused together to form casts. It may, therefore, be taken as very probable that the matrix of the leucocytal casts in my cases was formed in the same fashion; and that it may therefore not have been of fibrinous nature. It may also be added that, if the casts were formed in this fashion, they may be taken as signifying not only inflammation of the glomeruli, but also a special (vesicular) alteration of the cells of the convoluted tubes; the occurrence of the spherules alone indicating that this alteration took place. It seems to be doubtful whether the epithelium of other than the labyrinth-tubules is liable to this alteration. Cornil and Brault think not; but other authors, as has already been said, suppose that the

¹ On p. 66 of their '*Histologie Pathologique Générale du Rein*,' they say, '*nous avons pu suivre pas à pas la formation des boules aux dépens des cellules des tubes contournés, jusqu'à leur chute dans les tubes urinifères où elles contribuent à la formation des cylindres.*'

change may occur also in the straight and in the collecting tubes, and in the epithelium of the Bowman capsules.

The more precise origin of the leucocytes must remain undetermined. They may have passed by diapedesis through the walls of the glomerular tufts or of the tubules; they may have resulted from proliferation of the cells lining these walls.

The part, played by proliferation of epithelial and other cells in the formation of leucocytes, seems to be still in question. Dr. L. Beale in this country has specially insisted upon a proliferation of epithelial cells as giving rise to leucocytes in inflamed structures. Frey, following Remak, took this view: after referring to the multiplication of cells by budding of the nuclei of the cylindrical epithelium of the rabbit's intestine (a process similar to that which had been observed by H. Meckel in 1854 in Insects) he said, 'So entstehen bei entzündlichen Reizungszuständen aus den verschiedenen Epithelialzellen des menschlichen Leibes die Eiterkörperchen, welche aus jenen befreit bald in gewaltigen Mengen Bestandtheile des schleimigen oder purulenten Sekretes zu werden bestimmt sind.'

There were, however, other morphological elements in the urine, derived, no doubt, from parts of the urinary tract distal to the convoluted tubes; and it may be suggested that leucocytes and albumen proceeded from the same parts. There was a considerable number of oval and spherical cells (*v.* Plate, p. 62) having the appearance of ordinary fat cells. Gluge, in 1850, figured precisely the same kind of cells. They contained a peripheral layer of readily staining granular material, somewhat heaped up towards one pole of the cell, the remainder of the cell being perfectly clear. Gluge called them fat cells. The cells, which I met with, did not darken when treated with osmic acid; they may have been cells which had undergone mucous metamorphosis, cells of which I failed to ascertain the source. There was also in several cases a very considerable amount of epithelium in the form of free cells, which had apparently come from the straight or collecting tubes of the kidney, from the renal pelvis, the ureter, the bladder. The pyriform, tailed, and other characteristically shaped cells of the mucous membrane of the renal pelvis, the ureter and bladder were, as matter of fact, scanty; but there was in some cases a great quantity of perfectly spherical or of spheroidal cells having nuclei most clearly and readily discernible, and in two or three cases a profusion of such cells, containing brown granules. Heller, in 1845, spoke of the urine in Bright's disease as always containing, with other things, pavement epithelium and epithelium from Bellini's tubes. The cells of the

former he described as 'sehr schön und deutlich rundlich oval, manchmal fast rund mit sehr deutlichen Kernen'; and he spoke of the cells from Bellini's tubes as containing yellow or yellow-brown granules. Cornil and Brault speak of the ovoid cells, having large nucleus and clear protoplasm, as occurring in subacute or chronic diffuse nephritis, and as being derived from the straight and collecting tubes. These two kinds of cells I met with, but only in a few cases in any quantity; the brown cells I found in only three or four cases. It is quite clear that these cells were derived from parts of the urinary tract below the convoluted tubes. And it may therefore, as I have said, be suggested that leucocytes and dissolved albumen came from the same parts. It requires, however, to be added that in no case were these morphological elements present in any quantity before the fourth week of illness or before getting up; and that in each case, in which they did appear, albumen and leucocytes, with leucocytal casts, had been found during the earlier stages; and therefore, though albumen may have been furnished in later stages of these cases from more distal parts of the renal tubes, it had certainly been furnished also from more proximal parts.

Again, it may be contended that the urine was not altogether clear when tested; and that epithelium from the kidney, ureter, or bladder, may have yielded the reaction with picric acid. It will, therefore, be worth while to mention certain facts which seem to show that this was not so. On several occasions urine, passed during the later recovering stages, contained simply epithelium from the parts above-mentioned, without leucocytes; but no reaction was obtained in these cases with picric acid. Frequently, too, I tested urine which contained simply a profusion of urethral and vaginal cells, without other morphological elements; but I did not obtain the very faintest shade of turbidity with picric acid. Neither did I obtain any in cold water in which desquamated epidermis had been agitated for some time, nor in urine, nor warm water in which such epidermis had been macerated as many as four days (though later the reaction was exhibited), no matter whether the reagent were applied to the filtered clear fluid or to the fluid in which these cells were floating. Hence I conclude that the above cells of themselves did not give the reaction. It would be idle to deny that soluble albumen was furnished from the sites whence these cells were shed: but, as I have just remarked, these

cells appeared, in several instances, in profusion in urine, which gave no precipitate whatever with picric acid; and, when these cells were present, there had always been in the earlier stages evidence of mischief in the proximal parts of the glomerulo-tubular systems, as revealed by the presence of narrow casts and hyaline spherules. I am disposed therefore to refer the albumen in chief part, at any rate, to these portions of the kidney.

It may be observed that I have not figured any epithelial casts. I met with very few; indeed, cells unequivocally renal were found in only a small number of cases, and then solely in late stages. Dr. George Johnson, in 1852, called acute Bright's disease 'acute desquamative nephritis'; and in 1882, when drawing attention to the two kinds of scarlatinal kidney, viz. acute desquamative nephritis and glomerulo-nephritis, he said that the former had, as characteristic features, the proliferation and the desquamation of the glandular epithelium of the convoluted tubes. He said that, though tubular desquamative nephritis and glomerulo-nephritis might exist apart, they often coexisted in various proportions in the same subject, the epithelial and other casts being frequently associated in varying proportions with the white cell casts of glomerulo-nephritis. If this be true, then, my cases were, in all likelihood, cases of the latter affection pure and simple. But, as I have shown, the cells of the convoluted tubes were affected, and the ground-substance of the casts was formed, in all probability, of material actually discharged into the tubes by these cells. If, then, epithelial casts are what Dr. Johnson considers them to be, i. e. fibrin, which in process of coagulation in the tubules has imprisoned cells shed from the walls of the convoluted tubes; why were there so few to be found in my cases? Either my cases were exceptional, or Dr. Johnson's theory as to the formation of epithelial casts admits of modification. And in this connexion there are several matters to be borne in mind. The cells of epithelial casts figured by Dr. Johnson are granular and rounded or oval bodies, with very defined outlines and distinct nuclei; cells, that is, unlike those of the convoluted tubes in health, but, on the other hand, very like those of the lower parts of the tubules. Again, in pathological states of the kidney involving the convoluted tubes, and especially if they be inflammatory, the borders of the cells of these tubes, at no time well defined, are very quickly lost (MM. Cornil and Brault);

and the cells form together a protoplasmic membrane, a plasmodium, we may say, after the fashion of that described by Metschnikoff as formed by fusion of leucocytes around the spores of a certain fungus, which penetrate the body of the water-flea: whereas fusion of the cells is not found to take place in the straight or collecting tubes. Moreover, though the nuclei in this plasmodium may proliferate, there has not yet been observed any tendency to the formation of independent cells in it; whereas the cells of the collecting tubes do multiply, so that even several layers of cells appear in ordinary cases of nephritis. The epithelial casts, moreover, are but rarely convoluted: but this is not perhaps a difficulty, for it is quite likely that the cast, pliable at first, is moulded afresh lower down in the straighter tubes. [In all of my examinations of urine I did not find more than five convoluted casts; and of these, one was exceedingly short, and the others were twisted for only a short part of their length.] On these grounds I incline to regard epithelial casts as casts of which, though the material of the ground-substance is formed generally in the more proximal parts, the epithelium is added in the more distal parts, of the glomerulo-tubular systems. The absence of such casts in the early stages of my cases may be attributable to the fact that in those stages the renal mischief was limited to the labyrinths: and the absence in the later stages (in which renal cells in some cases did appear in the urine) to the fact that the mischief in the labyrinths had then largely subsided; or to some difference in the constitution of the hyaline spherules or of the body of the casts, by virtue of which the latter were less adapted to taking up waifs and strays in their course outwards. Of the authors who have spoken against the idea of a desquamation from the convoluted tubes, there may be mentioned Frerichs, Southey, Cornil and Brault.

After balancing the various kinds of evidence adduced above, I have concluded that all or most of my patients, who manifested albuminuria, suffered from organic renal mischief,—from glomerulonephritis and some change in the convoluted tubes. Whether the change in these tubes were primary or secondary (an anaemic necrosis, resulting from insufficient supply of blood to them from the inflamed glomerular vessels) may be questioned: that it may have been secondary appears to be indicated by the fact that albumen and leucocytes were commonly found prior to the spherules.

It should, also, be repeated that in several cases there was a profuse desquamation from the straight and collecting tubes, with some slight loss of epithelium from the renal pelvis, ureter, and bladder. In hæmorrhagic scarlatina it appears that hæmorrhage may occur in almost any part of the body. It has frequently been observed to occur from the mucous membrane of the renal pelvis; and albumen may thus pass into the urine. I had only two patients with pronounced hæmorrhagic scarlatina, though I had a considerable number with it in less pronounced form. One of these two patients, a young man, lost blood by the bowel very profusely, and presented a large subcutaneous ecchymosis in the hypogastric and lower umbilical regions, from which there sloughed off a portion of skin of the size of the outstretched hand. This patient had renal mischief, as was shown by renal casts: he, however, so quickly and completely lost the albuminuria that I supposed the albumen and red blood corpuscles in the urine had come, at any rate in some part, from the transrenal tract.

I should here state that Dr. H. Willey expressed his opinion in the 'British Medical Journal' of 1887, that in the majority of cases of scarlatinal albuminuria there was no organic change in the kidney, and that there was 'no difficulty in diagnosing real Bright's from simple albuminuria': in this connexion he added, 'the amount of albumen differs, the arterial tension is diminished, and, I think, this reduction of arterial tension and the albuminuria are very closely associated, for the amount of urine secreted increases exactly as the albumen decreases, and I think this is due to vaso-motor actions, so that the albuminuria is a nervous and not a kidney symptom.'

II. The second of the questions asked above (p. 59) needs but short consideration. I have shown that the albuminuria was not, or not in any considerable degree, due to mischief on the lower side of the kidney, transrenal mischief. I have also shown that the albumen was accompanied by leucocytes, leucocytal casts, and spherules, the first in large quantity; and I have, therefore, concluded that there was actual organic renal change, of a sort sufficient to account for the albuminuria.

Much has, however, been said, of late, of physiological or normal albuminuria. The earlier observers looked upon albuminuria as abnormal in all cases: for instance, D. Cotugno, after referring to his discovery of albumen in the urine, wrote concerning the latter: '*Nemo ignoret coagulabilem non esse*': so also Cruickshank in 1794, Rayer and M. Solon in 1838. Authors of eminence have,

however, more recently asserted that albumen may be demonstrated in normal urine, if sufficiently delicate tests be employed, and the urine be first concentrated: of them may be named Leube, Kleudgen, and Posner. Senator follows von Wittich in supposing that albumen habitually transudes through the glomeruli, and that in health it is absorbed by the renal cells. There is some *a priori* ground for supposing that albumen normally appears in the urine in extremely minute quantity; that, in fact, there is in health an escape of albumen in the urine, as Berzelius found there was of mucus. It is, moreover, thought by some that albuminuria may assume larger proportions than those here referred to, and yet be non-pathological. Bright said that he found albuminuria in the majority of persons in apparent health. Leube, in 1878, found on examining 119 apparently healthy soldiers, that about 4 per cent. of them had slight albuminuria on rising in the morning, and a further 12 per cent. at midday, after a heavy march or drill; the albuminuria in the latter cases, however, disappearing with rest. Nunn found this symptom in 12 per cent. of 200 persons, Fürbringer in 11.5 per cent. of 61 children, Kleudgen in 44 per cent. of 32 nurses, the health in each case being to all appearances good. There are, indeed, several authors who look upon albuminuria, when occurring, as in these instances, in persons in whom there is no other symptom to direct attention to the kidneys, as being non-pathological, or, as some say, functional. Prof. G. Stewart also found albuminuria in about one-third of 505 apparently healthy males. It should, however, be remarked that in most of these cases the persons were adults, that many were soldiers, and that some were of considerable age (over 60 years); in many cases the urine examined had been passed after exercise, even arduous exercise.

Before speaking of these kinds of albuminuria as non-pathological, it would be necessary to consider the mechanical and the irritative causes that are known to excite this symptom, transrenal sources also for albuminuria, and, in addition, structural lesions, such as progressing cirrhosis, or the after-effects of some mischief incurred possibly years before from scarlatina, diphtheria, or other infection. Indeed, Dr. G. Stewart, after an exhaustive and impartial consideration of all the facts adduced, and though he himself found albuminuria, as I have said, in nearly a third of the persons examined (505), concludes that 'albumen, if present at all

in normal urine, is present in such extremely minute amount as to be barely discernible, or not discernible at all, with the most delicate tests, even after considerable concentration.' Of others, also, who hold to this opinion, many eminent names might be given. It is at any rate perfectly clear that, though some presumably healthy persons may exhibit albuminuria, a very decided majority of those who have been examined have not been found to excrete albumen in the urine; and that in every instance in which albuminuria does appear, there still remains the necessity of ascertaining, as far as possible, whether it be due to organic mischief, cisrenal, renal, or transrenal.

That inflammation of the kidney is a sufficient cause of albuminuria is certain, and in my cases there was strong testimony of its existence. There was, it is true, albuminuria in a considerable number of persons after getting up, in whom it appeared that inflammatory mischief had subsided; but the structures, which had been affected before getting up, had doubtless been left in a condition allowing of escape of albumen, even under slight variations of blood pressure. Moreover, the proportion of error, which may, owing to inclusion of cases of so-called non-pathological albuminuria, have been introduced into my statistics, is not likely to have differed much from month to month, so that the monthly sets of cases would still remain very fairly comparable one with another.

CHAPTER III.

ALBUMINURIA BEFORE 'GETTING UP.'

THE incidence of albuminuria in its relation to season has already been cursorily referred to. I now proceed to discuss this subject in further detail, devoting the present chapter to that portion of the first twenty-two days of illness which was spent in hospital, and, as I have before intimated, in bed and on milk-diet.

Attention may first be given to Charts V and VI (p. 81). The curves therein drawn show the varying percentages of patients of stated ages who manifested albuminuria of specified degree during consecutive two-day periods, the same patient being represented but once in each such period. At the foot of the charts are given the actual numbers of patients examined and represented in the curves; they afford a sort of gauge for ascertaining the value of the percentages expressed. The charts embrace all the cases examined in October, November, December, and January, those belonging to September being omitted for reasons already stated. Chart V takes cognizance of all degrees of albuminuria except the slightest (shade ¹ and shade ²); the latter indeed, if included, would obliterate many or most of the depressions in the curves. The other chart takes account of only the larger amounts of albumen passed, albumen, that is, of or above a degree represented by 'trace' as defined in the first chapter. This difference, as regards the construction of the two charts, it will be necessary to bear steadily in mind.

It will be apparent at once, on examining these charts, that the incidence of albuminuria differed considerably among persons of different ages. For instance, in Chart V, the curve for persons who were over nineteen years of age, runs almost continuously at a higher level than the other curves; and that for persons under four years runs almost as regularly below all the rest: the curves for other ages taking a course intermediate between these, and here

DAYS OF ILLNESS.

PERCENTAGES

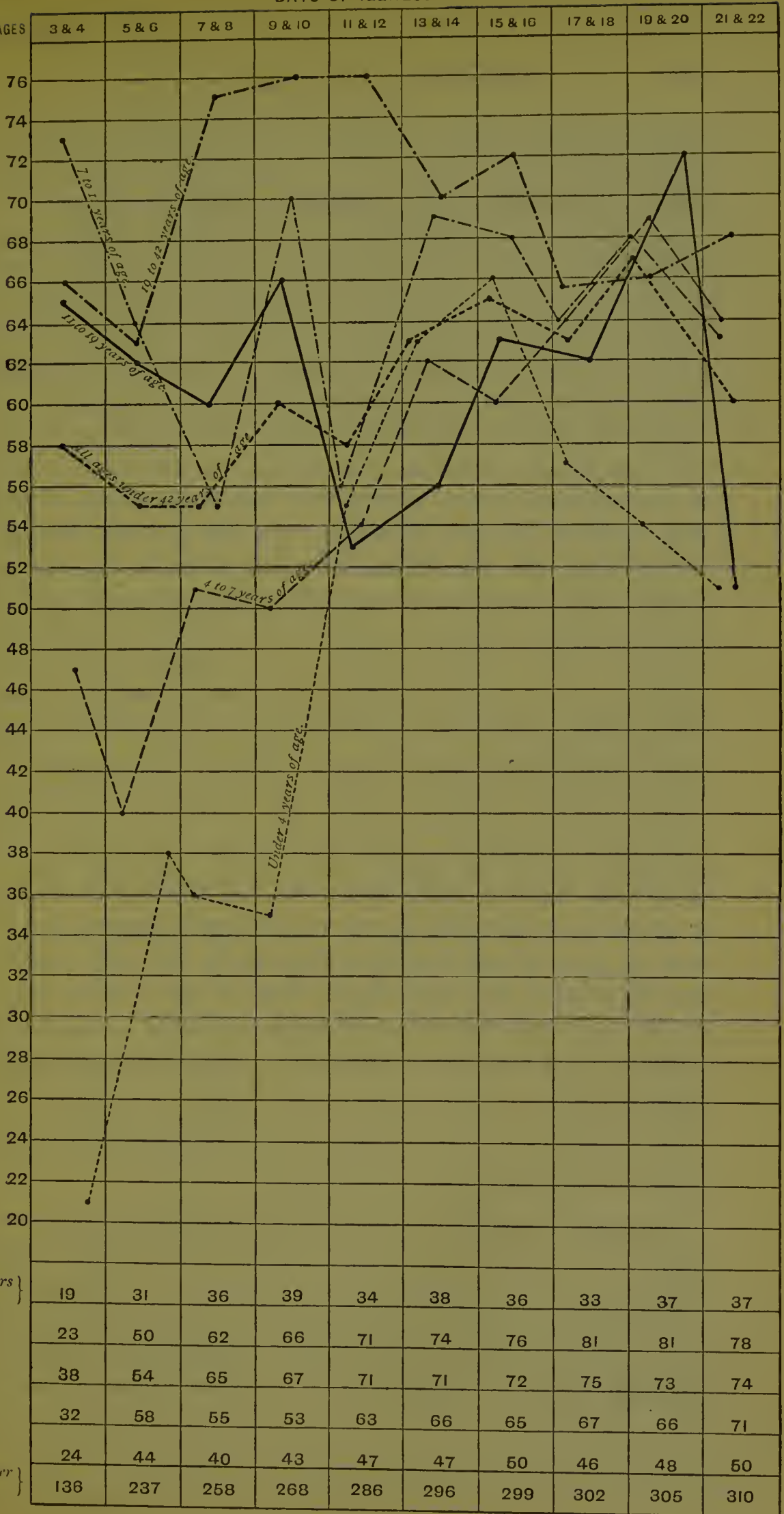


CHART V.—Showing, for different age-groups, the percentages of patients who on specified days of illness had albuminuria of degree above that represented by 'shade 2,'

Number of patients from whom the several percentages were estimated.

and there intersecting one another. The influence of age would appear, therefore, to have been considerable. I am not able to affirm the occurrence of any difference as between the sexes.

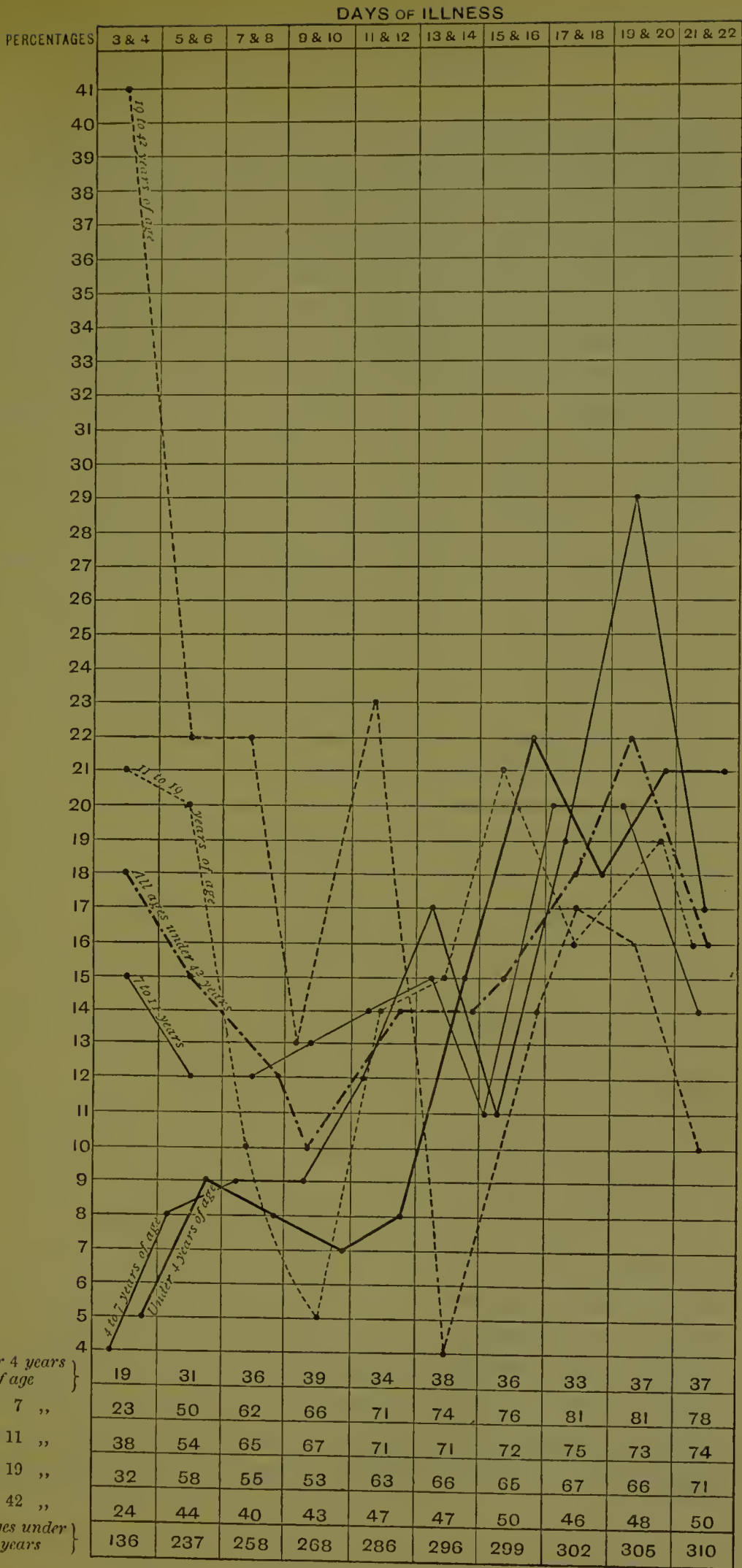
In regard to stage of illness and, as I shall later show, to season, the apices on these curves are of much interest. The curve for all ages in Chart V presents four main apices which appear severally for (1) the third and fourth days, (2) the ninth and tenth days, (3) the fifteenth and sixteenth days, (4) the nineteenth and twentieth days; and, speaking generally, it may be said that the curves for each age-group show similar apices. It will be remembered that variations were pointed out also in the pulse-rate at four different stages during the first three weeks of illness: they occurred on the following days, viz. (1) the first (initial acceleration), (2) the fifth, sixth, and seventh, (3) the tenth, twelfth, and thirteenth, and (4) the seventeenth and eighteenth; and thus they anticipated somewhat the occurrences of albuminuric variation. There were, moreover, three stages among persons between 4 and 19 years of age when irregularity of pulse was most common: they were (1) the ninth, (2) the fifteenth, and (3) the eighteenth to twentieth days of illness. I revert, however, to the albuminuria. As already said, Chart V shows that the relative frequency of albuminuria among persons over 19 years of age was greater, and that among persons under 4 years of age it was much less, than at other ages; but, on turning to Chart VI, it will be seen that albuminuria, of considerable degree, in persons over 19 years of age was more frequent in the early stages, and less frequent in the later stages, than at ages below 19 years. It will be noted also that, with some exceptions, the apices seen in Chart V are also seen in Chart VI. The apex on the nineteenth and twentieth days on the curve for all ages is to be seen in most of the curves in the two charts. The fall on or about the ninth day in Chart VI occurs somewhat later, as would be expected, than the first fall in the curves of Chart V.

What is the meaning of these elevations and depressions of the curves? The general coincidence between the several curves of each chart, and that between the curves of the two charts appear to show that the elevations and depressions are expressions of natural variations; and that the stages when albuminuric manifestations are most likely and least likely to occur are, speaking generally,

CHART VI.—Showing percentages of patients who on specified days of illness had albuminuria of degree either at or above that represented by 'trace' as defined in the text.

Number of patients from whom the several percentages were estimated.

Under 4 years of age }
 4 to 7 " }
 7 to 11 " }
 11 to 19 " }
 19 to 42 " }
 All ages under 42 years }



the same for most ages. The variations in the several curves cannot, then, be due to differences of age; neither are they due to differences of sex. It can in fact be shown that the variations were intimately associated with season. [As already said, if every grade of albuminuria were included in the charts, the curves would approximate to horizontal lines: and thus it is seen that the actual number of persons at different ages, who manifested albuminuria, did not differ so much as the charts would appear to represent; the variations in the curves, it may be repeated, are differences of degree rather than of kind.] The variations in the curves show that the albuminuria varied in the course of the individual's illness, or that it appeared at different stages of the illness in persons attacked at different dates. It is true that in the albuminuric curves for the individuals there were variations (elevations and depressions); but in most of these curves there was but one rise and fall; and other rises and falls, when present, did not occur at the same stages in all cases so as to explain those seen in the charts. It was therefore necessary to see whether individuals attacked in different months showed any great differences in the stages of illness when they were most liable to albuminuria. An examination of the curves, with this object, was accordingly made. They were found to belong to one or other of three different classes; viz., (1) in which the albuminuria came on early and fell early, (2) in which it came on in the early part of the second week and continued for a moderate or a considerable time, and (3) in which it came on late and continued generally for a long time. Altogether as many as 316 of the total of 347 patients examined could be definitely placed in one or other of these three classes, and 16 of the remainder had no albumen in the urine at any time. Of these 316 there were 154 who fell ill in October, 65 in November, 61 in December, and 36 in January: and 60.3 per cent., 53.8, 54.9, and 38.2 per cent. of these patients respectively belonged to the second class above mentioned in regard to albuminuria; so that the October patients are chiefly responsible for the second rise in the curves of Chart V. The corresponding percentages for the first class were as follow, viz., 37.6, 44.6, 31.1, and 30.5; so that October and November patients were especially concerned in bringing about the first apex. I am hereby led to suppose that the various elevations and depressions in the albuminuric curves

are expressions of seasonal variations. It may be asked whether the variations of rate of pulse and of irregularity of pulse, already mentioned at page 34, were explicable on the same or similar grounds. It is true that irregularity of pulse was more common among those patients who were attacked in October and November than among those attacked in December and January; but the irregular pulse showed a tendency to recur at fairly definite stages in the course of the illness in the individual, whether attack had taken place in one month or another; and the like was true of the variations of rate of pulse: in other words, neither of these two kinds of variation depended wholly upon conditions of season. Dr. S. Ringer found that in scarlatina there was a special tendency for the temperature to fall on the fifth, the tenth, or the fifteenth day; this led him to suppose that the temperature in health was subject to variation every fifth day; the temperature in scarlatina tending to fall on one or other of those days, just as in pyrexia it generally falls towards early morning. Dr. Ringer did not entertain the view that these thermal changes in scarlatina were possibly related to a periodicity in the mode of action of the virus at certain intervals of time. I would not definitely say that such is the case; but my results do seem to point to the operation of some such cause for the variations of pulse-rate and, in certain instances, for those of albuminuria in the individual.

I have, however, spoken, as if albuminuria, whether occurring at one time or another in the course of scarlatina, were of essentially the same nature. It has been so regarded by several authors, but others have looked upon early albuminuria—albuminuria in the first week or so of the illness—as being of a sort altogether separate and distinct from that which occurs later: and whereas most of those who have taken this view have thought early albuminuria to be due or intimately related to pyrexia, Warburton Begbie thought it resulted from a process of desquamation of the renal epithelium analogous to that of the cuticle; all, however, ascribing later albuminuria to inflammation set up either as a specific process or as a result of chilling. Of the various interpretations given to early albuminuria, perhaps that which ascribes it to pyrexia has met with widest acceptance, a view which may be said to date from the time when Gerhardt first drew attention to ‘febrile albuminuria’; though, it may be remarked, in passing, previous

observers (Cruikshank, Nysten, Gregory, Darwall, and others) had spoken of albuminuria in febrile disorders.

'Febrile albuminuria' has been attributed to a great variety of immediately determining causes. Senator ascribes it to a greater diffusibility of the blood-albumen, owing to (1) mere elevation of temperature and (2) accumulation of urea and salts in the blood. Bartels thought it due to relaxation of the walls of the blood-vessels, as in the albuminuria following section of the vasomotor nerves of the kidney; and, as matter of fact, Senator has found that relaxation of all the arteries of the body, especially of those of the viscera, does occur in pyrexia. Mahomed attributed it to increase of venous, with diminution of arterial, pressure. Some, again, have thought it may result from cloudy swelling and other changes in the renal tissue; and yet others have ascribed it to irritation of the urinary passages, owing to a concentrated state of the urine.

It will therefore be necessary to consider more carefully the view which I have taken of early albuminuria in my cases.

The facts concerning this albuminuria are given in Charts V and VI. From Chart V it will be seen that it fell off on the fifth and sixth days among the patients belonging to four different age-groups; for two of these groups the fall was still more marked on the two following days, when also the fall for the other (the fifth) age-group took place. After this primary fall, the curve in each case quickly rose higher than before.

Before discussing the signification of early albuminuria, it may be well to review the opinions advocated by Steiner and Eischitz on the one hand, and by Thomas and Bartels on the other. The former two supposed that scarlatinal albuminuria, whether of early or of late occurrence, signified renal mischief of the same sort though not of the same intensity; that, in fact, the albuminuria commenced as a result of slight renal catarrh, which, developing further, terminated in marked inflammation. Steiner took this view, because he found the kidneys of scarlatinal children, who had died on the second or third day, already¹ hyperaemic and

¹ It may also be said that Dr. Copland in 1858 wrote: 'The kidneys may become affected in a very prominent manner early in the course of the disease: indeed, I believe them to be always more or less affected at an early period, although this affection has been overlooked at this period, and recognised only during the process of desquamation and recovery.'

enlarged, and the secreting epithelium of all such kidneys to be cloudy. Professor Thomas, on the other hand, writing in Ziemssen's 'Cyclopaedia,' spoke of early albuminuria as of quite secondary importance, as a febrile albuminuria, and in no way related to albuminuria of later stages. In his experience the symptoms of renal catarrh at the outset of the disease occurred only rarely; when they did occur, they generally diminished or wholly disappeared with the decline of the pyrexia in the second week; and, despite such disappearance, the symptoms of parenchymatous nephritis appeared later, at times quite suddenly, between the thirteenth and twenty-first days. He also ascribed to pyrexia the albuminuria of other infectious, and indeed of non-infectious, febrile diseases; for in them he found the same evidence of slight renal catarrh (the same cloudy swelling, and the same casts, epithelial, hyaline, and cylindroidal) as in the early stages of scarlatina. Nevertheless, Thomas was not fully satisfied with this view, because in these other diseases he did not find renal catarrh so¹ frequent as in the early stages of scarlatina; and on this ground he suspected that the early renal catarrh of scarlatina was due to a specific irritation as well as to the pyrexia, now in the main to one, now to the other. Bartels² also, writing in 1875 in his 'Handbuch der Krankheiten des Harnapparates,' followed the views expressed by Gerhardt: he found in a certain scarlatinal epidemic, in 1853-54, that albuminuria generally occurred when the rash was fully developed, and that it disappeared as the fever subsided;

¹ 'Nur mag die Regelmässigkeit des Erscheinens des Harnkanälchenkatarrhs bei diesen Krankheiten geringer als beim Scharlach und daher der Schluss erlaubt sein, dass der Scharlachkatarrh durch die specifische Reizung sowohl wie durch das Fieber hervorgerufen werde. Im Einzelfall dürfte das grössere Gewicht bald mehr auf das Eine, bald das Andere zu legen sein' (l. c., p. 231).

² 'Man muss sich aber hüten, die Albuminurie verursachende Wirkung des Fiebers mit dem specifischen Einflusse des Scharlachprocesses auf die Nieren zu verwechseln. Febrile Albuminurie kommt bei schweren fieberhaften Scharlachkranken ebenso wie bei allen anderen schwer fieberhaft Kranken vor; sie fällt aber mit der Höhe des Fiebers zusammen und verliert sich mit dem Fieberabfall. Die Nephritis folgt dem Scharlach erst nach einer meistens ganz fieberlosen Periode von 1 bis 2 Wochen Dauer nach, wenn das Exanthem längst verblasst ist' (l. c., p. 221).

'Ausdrücklich aber unterscheide ich die acuten diffusen Nierenentzündungen, welche bei Diphtheritis, Recurrens und nach Scharlachfieber so häufig auftreten, von der febrilen Albuminurie. Es handelt sich dabei um specifische Einflüsse auf die Nieren, welche mit der etwa begleitenden oder vorausgegangenen Fieberhitze unmittelbar Nichts zu thun haben' (l. c., p. 48).

but that, nevertheless, nephritis came on later, about the twentieth day. Still, despite his unqualified statements regarding early albuminuria in scarlatina, Bartels was not prepared to ascribe to pyrexia the transitory albuminuria which he frequently met with in a certain¹ epidemic of small-pox; and he made the like reservation in regard to diphtheria². It deserves notice, however, that the above authors did not assign to glomerulitis the frequency of occurrence and the importance that have since been claimed for it: they all looked upon scarlatinal nephritis as essentially, and from the first, an affection of the tubules.

Glomerulitis has, however, as I have said, been found to be very generally the primary mischief in the kidney; and the changes in the tubules have been commonly regarded as secondary to it: it may therefore well be that glomerulitis has been largely overlooked, both during life and after death. Moreover, Klein found both glomerulitis, and changes in the arterioles of the kidney, as early as the second day of scarlatina; and it may be mentioned that M. Bouehard has found evidence of nephritis in enteric fever on the third day, with bacteria in the urine and in the kidney. Violent symptoms of nephritis do not, it is true, arise in scarlatina frequently before the third week; but they do occur in the first week, as the case recorded by Dr. Klein, and two of my own patients, serve to illustrate. It is, then, quite evident that early albuminuria of scarlatina *may* be due to actual renal lesion of precisely the same nature as late albuminuria. Still, we are con-

¹ 'Indessen will ich ausdrücklich bemerken, dass die bei Pocken in unserer letzten Epidemie so sehr häufig beobachtete vorübergehende Albuminurie allerdings keineswegs in allen Fällen als Fiebererscheinung gedeutet werden konnte, vielleicht also durch spezifische Veränderungen der Gefässe in den Nieren bewirkt wurde. Makroskopisch wahrnehmbare Veränderungen der Nieren fanden sich aber in keiner der 43 in dieser Epidemie obducirten Pockenleichen, abgesehen natürlich von ein Paar Fällen, welche mit chronischen Nierenkrankheiten behaftete Individuen betrafen' (l. e., p. 224).

² 'Häufiger vielleicht, als bei Scharlach, kommt bei Diphtherie eine leichte Albuminurie ohne tiefgreifende Veränderungen der Nieren vor, so dass nach dem Tode des Patienten selbst genauere mikroskopische Untersuchungen der Nieren keine wesentliche Abweichungen von der normalen Beschaffenheit nachzuweisen vermögen, und dass in Genesungsfällen die Albuminurie schon nach ganz kurzer Zeit wieder verschwindet. Dennoch bin ich geneigt, auch diese Fälle von Albuminurie auf jenen spezifischen Einfluss zu beziehen, da sie sehr häufig weder durch hohe Fiebertemperaturen noch durch Stauungshyperämie erklärt werden können; diese kurzdauernde Albuminurie kommt nämlich auch bei sehr leichten Fällen von Diphtheritis vor, die weder hohes Fieber veranlassten, noch irgend welche Störungen der Respiration bewirkten' (p. 223).

fronted with the statements, made by Thomas, Bartels, and others, that there may be in the same individual both early and late albuminuria, the two being separated by an interval of some days, when there is no albuminuria. What is the meaning of this interval?

First, as to the facts. Statistical data have not on any large scale been recorded. Thomas and Bartels did not, I think, record any. Steiner and Eisenschitz would appear not to have allowed that such an interval occurred. Warburton Begbie, also, who in 1852 divided the albuminuria of scarlatina into desquamative and inflammatory, the former coming on three or four days after desquamation of the cuticle had begun, and disappearing in thirty-six hours to ten days, and the latter occurring later, said: 'in all my cases' (i.e. of inflammatory albuminuria) 'the dropsical and aggravated symptoms appeared at the time when the temporary' (i.e. the early or desquamative) 'albuminuria was going on, and they were evidently the result of exposure to cold': in other words, though he drew a decided distinction between the early and late albuminuria, at least in their causation, he had not observed the occurrence of any interval between them. Further, among my patients of all ages, a comparison of the proportion of those, who exhibited any but the slightest degree of albuminuria on the third and fourth days, with the proportion of those, who exhibited such albuminuria on the fifth to the eighth days, shows that there was a reduction of only 3 per cent. (*v.* Chart V). In no case, however, did the albuminuria wholly disappear between the fifth and eighth days, though in some it diminished very greatly in amount; and the fall, shown in the Chart, was due, not to the total disappearance, but to the great diminution of the albumen passed by the individual patients. In fact, in none of my cases did the albuminuria wholly disappear until some days afterwards. And, though in a few instances it *increased* still later, it but very rarely *reappeared* after it had once wholly disappeared. Finally, there are the data which have been furnished by Dr. R. S. Thomson¹. Dr. Thomson examined the urine of 180 consecutive scarlatinal patients three times daily, from the time of admission to hospital until at least the fifty-sixth day of illness. Of these there were 112 who

¹ *Ibid* 'Transactions Royal Med. and Chirurg. Soc. London,' for 1886.

had albuminuria; 40 of them had it early in the illness. Referring to these 40 patients, Dr. Thomson found that in 10 there was no albuminuria after the first week; in 9 the early albuminuria continued *without a break* into—and in 21 it was followed, after a varying interval, by—late albuminuria: and he would lay stress on the frequency of the occurrence of an interval between early and late albuminuria. It is difficult to give a precise estimate of the value of these figures. In my cases the stage of illness when the fall of early albuminuria took place varied somewhat with age; it varied also with season: and in Dr. Thomson's cases, reference is not made to these matters. As regards age, it may be seen in Chart V that with persons at ages over nineteen, and also between four and seven years, the second rise had already begun by the seventh or eighth days, whereas at other ages it did not begin until later; and on turning to Dr. Thomson's cases, it seems that for three of the nine patients, in whom early albuminuria continued without a break into late albuminuria, this symptom was not found before the seventh evening. Again, of the twenty-one patients, in whom early albuminuria was followed, after a varying interval, by late albuminuria, two appear in the notes to have been admitted as late as the twenty-first day, and three of them were not admitted before the sixth day. And, further, the interval between early and late albuminuria observed by Dr. Thomson—an interval, it is to be noted, of absolute freedom from albuminuria—was no longer, and was in many instances very much shorter, than intervals which were found at later stages of the disease. Two examples may be quoted, (1) in which early albuminuria ran on, without a break, into late albuminuria, and (2) in which it was followed, after an interval, by late albuminuria. Albumen is reported for the first case at the following times, viz. third evening, fourth evening, eighth evening, eleventh morning, seventeenth evening, and eighteenth evening; and for the second case, on the fifth day, the ninth evening, and the twenty-ninth day; the urine having been free of albumen at all other times when examined, and it was examined thrice daily. Dr. Thomson does not say why he would lay more emphasis on the first interval than on intervals which occurred at later stages, such as are to be seen in the examples just cited. The presence of these marked intervals is further of importance, as it serves to show the great dissimilarity between my cases and those

of Dr. Thomson. In the second example given above, 'abundant albumen' was reported for the fifth day, then absolutely no albumen for three whole days, and then reappearance of albumen. Again, in most cases in which he found albuminuria he reported it as reaching or exceeding the degree called *trace* (though lower degrees are also at times mentioned), as lasting one day, two, three, four, or more days, and then as disappearing abruptly. In these respects Dr. Thomson's patients were not like mine, none of whom presented any abruptness in the cessation of albuminuria, i.e. during the period passed in bed, before getting up; and the fall at the end of the first week in my cases was one of degree rather than of kind.

Hence, as to the occurrence of an interval between early and late albuminuria, no very definite data have been recorded; and most of the statements that have been made concerning it have been wholly unaccompanied by references to the microscopical characters of the urine.

Nevertheless, it is very generally believed that there is such an interval; and we may pass on to discuss the interpretations offered for it. Are early and late albuminuria due to essentially the same renal lesion, the reappearance of it being of the nature of a relapse? Is the early albuminuria due to pyrexia, and the late albuminuria to a specific lesion of the kidney, as was held by Thomas, Bartels, and others? Is early albuminuria a mere associate of a desquamating process of the renal tubules, and late albuminuria a symptom of inflammation of the kidney due to chilling; as was supposed by Warburton Begbie? Having regard to the great diversity in the several manifestations of scarlatina, I hesitate in formulating any theory as to scarlatinal albuminuria in general; and arguments, which I now proceed to bring forward, are to be considered as having special reference to my own cases.

First, as to the pyrexial theory of early albuminuria. Liebermeister supposed that a body-temperature of 104°F . or more was necessary to the production of heat-albuminuria; and Senator finds that albuminuria comes on in dogs when the body-temperature is raised artificially by 3.4 to 6.7°F . with sufficient rapidity. It appears, however, that of my own patients attacked in or after October, those who suffered from albuminuria on the third and fourth days formed 58 per cent. (*v. Chart V*); whereas those in

whom the temperature reached 103°F . or more at any time during the first week of illness while in hospital, either in the morning or in the evening, did not form 25 per cent.; though it is probable that prior to admission this percentage was higher. Again, of the December patients, one had a maximum temperature within the first week of illness of 104°F ., and two others had a maximum temperature of 103.6°F .; but in the urine of none of these three did I find the slightest turbidity on adding picric acid. Again, the first fall in the curve for all ages in Chart V is not so marked as might be expected if the albuminuria had been due to temperature, 55 per cent. of the patients having albuminuria, notwithstanding the fall. There was, in the early stages, a much more pronounced fall in the percentage of patients who suffered from the higher degrees of albuminuria, as shown in Chart VI; but this fact only serves the more to call attention to the matter as one of degree rather than of kind.

My statistics would seem, therefore, to show that there was a greater frequency of early albuminuria than the temperatures might be supposed to account for. Dr. Thomson's statistics would, however, perhaps more nearly accord with such an interpretation. Early albuminuria was present in 40 of Dr. Thomson's patients, i.e. in 22 per cent.; and, if the temperatures were of much the same intensity as those of my patients, the facts would afford some support to the pyrexial theory: and, seeing that among the 40 there were included four for whom albumen was not recorded before the eighth day, seven for whom it was not recorded before the seventh day, and four not before the sixth day; and that, concerning two others, the notes refer to them as having been admitted on the twenty-first day; it is possible that the proportion of patients with early albuminuria was not so high as 22 per cent.; and this would perhaps bring the pyrexial theory and the facts concerning Dr. Thomson's patients into still closer correspondence.

It would appear, then, impossible, as yet, to ascertain whether and how far early albuminuria in scarlatina is necessarily related to pyrexia. The pyrexial theory certainly does not satisfactorily explain my cases. Early albuminuria occurred, though the temperature was only slightly raised; and it was absent, though the temperature was very considerably raised. Nephritis, moreover, may, there is no doubt, occur even in the first half of the first

week of scarlatina, and of a sort precisely such as occurs at later stages; and—a matter I would especially insist on—such nephritis, so far from being necessarily fatal, may be attended by scarcely any symptoms, its presence, indeed, being indicated only by leucocytes, hyaline spherules, and leucocytal casts in the urine. Dr. Begbie ascribed early albuminuria to escape of albumen from the renal tubules in process of desquamating, independently of any inflammatory process, because it commenced very shortly after the desquamation of the cuticle, and because it was not associated with febrile reaction, lumbar pain, or any change in the urine, save the presence in it of albumen and renal, as well as other, epithelial cells. In my cases albuminous urine of the early stages contained only a very little renal epithelium; but, on the other hand, it did contain, as I have said, a large quantity of leucocytes, hyaline spherules, and occasionally leucocytal casts. Moreover, though febrile reaction and lumbar pain may be associated with inflammation of the kidneys, it is certainly a fact that, in the latter affection, there may be neither the one nor the other.

I have therefore concluded that in my patients early albuminuria was not due to pyrexia *qua* pyrexia; and I have been inclined to attribute it, as well as the albuminuria of later stages, the one and the other, to the same sort of mischief, each being as much an essential part of the train of determinations in scarlatina as the tonsillitis, the rash, the cervical adenitis, or other determination.

CHAPTER IV.

ALBUMINURIA ON FIRST 'GETTING UP.'

IN this chapter I shall consider the meaning of albuminuria when exhibited, as it was in very many instances, on first getting up, by persons whose urine, though previously albuminous, had for some days before getting up been apparently normal in all respects, save perhaps in quantity. Such cases have been referred to by several authors. I had a large proportion of them, as shown in Table T.

The albuminuria appeared frequently in the course of two or three hours, in some instances not till two or three days, after the patient had begun to get up; in others it appeared later. The albumen varied greatly in amount (a shade, a trace, a flocculency), though the urine had been free of albumen for several days before the patient got up.

So long as the albumen, passed while the patient was erect, did not exceed the grade called shade³, the patient was allowed to continue to get up daily; and in such cases the albuminuria generally disappeared in the course of a few days. Of the other patients, confined to bed for the reason just intimated, some continued for a few days and nights while in bed to pass albumen, though in rapidly diminishing amount; but most of them lost the albuminuria still more quickly. After being thus free for some days, they were allowed to get up again: albuminuria then in some cases recurred; and, while the patient still continued to get up, it either gradually disappeared, or again became of such degree as to necessitate another confinement to bed. Food (whether milk, fish, or meat) seemed to have no influence on this albuminuria, except in a very few instances, in which the renal mischief had been more than moderately severe.

This recurrence of albuminuria, on getting up, was seen among patients of both sexes and of many different ages. In the progress, moreover, towards complete disappearance of this albuminuria, there was commonly a stage, when the symptom appeared within half an

hour after getting up, and disappeared within as short an interval after lying down. Similar appearance and disappearance of albuminuria occurred in cases related by Drs. Bence Jones in 1850, Bartels in 1872, G. Johnson in 1873, and Mahomed in 1874.

Mahomed's patient had suffered from scarlatinal albuminuria for a period of three months, when, according to the record, 'the interesting fact was discovered that the albuminuria only occurred when' the patient 'was up. This change was instantaneous, half an hour in either condition sufficing to alter the character of the urine, and corresponding to this altered secretion there was a changed condition of the circulation' (revealed by the sphygmograph) 'on which it depended.' Mahomed found a large decrease of urea in the urine passed while the patient was erect, as compared with that passed while in bed, when the urine was normal; and this, though the diet on each occasion was exactly similar; and he accounted for this by supposing that the capillary circulation was retarded (as shown by the sphygmograph), and therefore the tissue-change, when the patient was erect. (Trans. Roy. Med. and Chi. Soc. Lond., 1874.)

Intermediate gradations of all degrees were to be observed between these different cases; and it may be of interest to consider

TABLE S.—*Showing the number of October- to January-patients who 'got up' on successive days between the twenty-first and thirty-first.*

	Day of illness.											
	21st	22nd	23rd	24th	25th	26th	27th	28th	29th	30th	31st	21st to 31st
Under 4 years of age	0	0	0	0	2	5	3	3	1	0	4	18
4 to 7 „	0	0	1	2	4	8	5	3	5	5	3	36
7 „ 11 „	0	3	3	5	6	5	2	8	5	2	3	42
11 „ 19 „	0	1	1	6	7	9	10	4	4	5	5	52
19 „ 42 „	0	0	0	4	6	4	5	1	5	4	2	31
	0	4	5	17	25	31	25	19	20	16	17	179

them further. In Table S. I have put together the numbers of patients, who got up for the first time on successive days from the twenty-first to the thirty-first of the illness. Only 9 it appears got up on or before the twenty-third day; 170 got up at times varying from the twenty-fourth to the thirty-first day. In order not to complicate matters by introducing wide differences of date of illness when the patients got up, I shall confine attention to these 179 patients. Albuminuria of varying degree recurred in 130 of these;

it did not reeur in 45 ; and in 4 instances the urine was not examined ; other details are given in Table T.

I have already intimated, in Subsee. A. Chap. II, that season had to do with this phenomenon. Age also had some influence. Persons under four years of age suffered (*vide* Table T.) in smaller proportion ; and they, with those of four to seven years of age, suffered in smaller degree, and with less persisteney, than persons above these ages. Those affected most with large increase, and with persisteney of albuminuria, were of ages from seven to nineteen years ; facts in aeoord with what has already been shown concerning the incidence of albuminuria generally.

TABLE T.—*Showing statistics concerning reappearance of albuminuria in October- to January-patients who ‘got up’ on or before the thirty-first day.*

	Number of patients who got up on or before the 31st day.	Percentage of persons in whom albumi- nuria reappeared after getting up.				Average number of days that elapsed, after the patient first got up, before albu- men totally disappeared.
		Total.	And in whom the albuminuria was			
			Very slight.	Slight.	Con- siderable.	
Under 4 years of age	18	66.6	22.2	38.8	5.5	10
4 to 7 ,,	36	72.2	30.5	38.8	2.7	12
7 ,, 11 ,,	42	78.5	28.5	35.7	14.2	15
11 ,, 19 ,,	52	71.1	11.5	25.0	34.6	20
19 ,, 42 ,,	31	70.9	25.8	35.4	9.6	14

What is the meaning of this recurrence of albuminuria ? Where it continued for some days after recommitment to bed, renal changes had doubtless been set up afresh. Where it wholly disappeared in the course of three or four days, although the patient was continuing to get up, there was not, it may be supposed, any active change newly stirred up in the kidney. And in these cases, as also in those, in which albuminuria appeared for the most part only while the patient was erect (coming on very shortly after getting up, and disappearing as quickly after lying down), the immediately determining cause would seem to have been of an essentially mechanical nature.

The intimaey of the relationship between posture and this form of albuminuria, which for the present I shall, with Dr. Stirling,

call 'postural,' may be gathered from Table U. From this table, in which all the urine, passed by the patients referred to, is represented, it will be seen that the total urine, passed during the first or second two-hour period after lying down, did not give an albumen-reaction with picric acid; whereas that passed during the two hours after getting up did. In cases thus closely related to posture morphological elements were but rarely observed in the urinary sediment. There were occasionally a few leucocytes, a few renal cells perhaps, but no other formed bodies, organic or inorganic; sediment indeed was all but absent. Neither the reaction for peptones, nor that for sugar, was found in any urine, whether it were passed when the patient was erect or when recumbent. There had, however, been in the history of each case of postural albuminuria a prior stage, in which the urine contained a profusion of leucocytes, some leucocytal casts and hyaline spherules (elements which were present in other cases also, in which the assumption of the erect posture played a very active part in bringing on albuminuria). Hence postural albuminuria was presumably renal: if it had been transrenal the loss of albumen would probably have been continuous, and the patient would probably not have become so anaemic as was the case with some of my patients.

Let us now discuss the factors likely to be thought of as having had causal relation to the phenomena in question. Most of the data, to which I shall appeal, are set out in Tables U and V.

1. *Volume of Urine.* It has by some been stated that the volume of urine passed during the erect posture in health is greater than that of the urine passed during recumbency. Bartels spoke to the same effect. In all the cases, also, in which I estimated the volume of urine of non-albuminuric convalescents for two consecutive periods, each of twelve hours, one period (8 p.m. to 8 a.m.) spent in bed, and the other (8 a.m. to 8 p.m.) while erect, I found this to be so. Thus:—

Henry S.,	21 years of age,	in bed passed	17	ozs., while up	24½	ozs.
"	"	"	17	"	33	"
Chas. H.,	36	"	16⅔	"	27	"
Jas. D.,	34	"	21½	"	24	"
George A.,	37	"	21¹	"	29	"

It was the same with some of the patients, who had postural albuminuria, e.g. Table V,—William M., IV, Richard B., VII,

TABLE U.—*Showing appearance of albumen in, and disappearance*

Time.	Conditions as to posture.	Urine.			
		Albumen.	Clear or turbid.	Reaction.	Colour.
Wm. M., 8 years.					
Feb. 10, midnight	In bed	None	Clear	Acid	
Feb. 11, 2 a.m.	"	"	"	"	
4 a.m.	"	"	"	"	
6 a.m.	"	"	"	"	
8 a.m.	"	"	"	"	
10.30 a.m.	Getting up	"	"	"	
noon	Going to bed	Trace	"	"	
2 p.m.	In bed	None	"	"	
4 p.m.	"	"	"	"	
6 p.m.	"	"	Urates	"	
8 p.m.	"	"	Clear	"	
midnight	"	"	"	"	
Feb. 12, 4 a.m.	"	"	"	"	
6 a.m.	"	"	"	"	
8 a.m.	"	"	"	"	
9 a.m.	Getting up	"	"	Slightly acid	
10 a.m.	Up and dressed	Shade ¹	"	Acid	
noon	"	Trace	"	"	
2 p.m.	"	Shade ¹	"	"	
4 p.m.	"	Shade ²	"	"	
6 p.m.	Going to bed	Shade ³	Urates	"	
8 p.m.	In bed	Shade ²	Clear	"	
10 p.m.	"	None	Urates	"	
midnight	"	"	"	"	
Feb. 13, 2 a.m.	"	"	"	"	
4 a.m.	"	"	"	"	
6 a.m.	"	"	"	"	
8 a.m.	Getting up	"	Clear	"	
10 a.m.	Up and dressed	Trace	Urates	"	
11 a.m.	Just got into bed	Shade ³	"	"	
noon	In bed	None	Clear	"	
2 p.m.	"	"	"	"	
3 p.m.	Getting up a 2nd time	"	"	"	
4 p.m.	Up and dressed	Trace	"	"	
6 p.m.	Just got into bed	"	Urates	"	
8 p.m.	In bed	None	Clear	"	
10 p.m.	"	"	"	"	Orange
midnight	"	"	Urates	"	Pale orange
Feb. 14, 2 a.m.	"	"	"	"	Orange
6 a.m.	"	"	"	"	"
8 a.m.	Getting up	"	"	"	Very pale orange
10 a.m.	Going to bed	Trace	"	Slightly acid	Pale orange
10.30 a.m.	In bed	None	"	Acid	Pale straw
11 a.m.	"	"	Clear	"	"
11.30 a.m.	"	"	"	Neutral	"
noon	"	"	"	"	Colour of water
Ellen H., 7 years.					
Feb. 18, 11 a.m.	Getting up	None			
5 p.m.	Going to bed	Shade ²			
7 p.m.	In bed	None			
Feb. 20, noon	Getting up	"			
2 p.m.	Going to bed	Trace			
4 p.m.	In bed	None			

of albumen from the urine in relation to Posture.

Time.	Condition as to Posture.	Urine.			
		Albumen.	Clear or turbid.	Reaction.	Colour.
Chas. R., 12 years					
Feb. 14, 8 p.m.	Been in bed 4 hrs.	None	Turbid	Alk.	Straw
10 p.m.	In bed	"	"	Neutral	Pale orange
midnight	"	"	"	"	"
Feb. 15, 8 a.m.	"	"	"	"	Straw
10 a.m.	"	"	"	Alk.	"
noon	Getting up	"	"	Neutral	"
2 p.m.	Up and dressed	Trace	Clear	Acid	Pale orange
4 p.m.	Going to bed	Decided trace	Turbid	"	"
6 p.m.	In bed	Shade ³	"	"	Straw
8 p.m.	"	Shade ¹	Clear	"	"
10 p.m.	"	None	Turbid	Neutral	Pale orange or straw
Feb. 16, 4 a.m.	"	"	"	"	Dark orange
10 a.m.	"	"	"	"	Straw
noon	Getting up	"	"	"	"
4 p.m.	Going to bed	Shade ³	"	"	Pale straw
8 p.m.	In bed	None	"	"	Straw
10 p.m.	"	"	"	"	Brown
Richd. B., 15 years					
Feb. 14, 6 p.m.	Been in bed 2 hrs.	None	Clear	Neutral	Straw
8 p.m.	In bed	"	"	Acid	Pale straw
10 p.m.	"	"	Turbid	Neutral	Orange
Feb. 15, 8 a.m.	"	"	Clear	"	Straw
10 a.m.	"	"	Turbid	"	Orange
noon	Getting up	"	"	"	"
2 p.m.	Up and dressed	Trace	"	Acid	Pale
4 p.m.	Going to bed	"	Clear	"	Orange
6 p.m.	In bed	None	Turbid	Neutral	Brown
8 p.m.	"	"	"	"	Straw
10 p.m.	"	"	"	"	"
Feb. 16, 4 a.m.	"	"	"	"	Dark orange
8 a.m.	"	"	Clear	Acid	Straw
10 a.m.	"	"	"	"	Pale orange
noon	Getting up	"	"	Neutral	Water colour
4 p.m.	Going to bed	Shade ²	"	Acid	Straw
8 p.m.	In bed	None	"	"	"
10 p.m.	"	"	Turbid	Neutral	Dark orange
Feb. 17, 4 a.m.	"	"	"	"	"
6 a.m.	"	"	"	"	Straw
10 a.m.	Getting up	"	Almost clear	"	"
Fred. W., 19 years					
Feb. 15, noon	Getting up	None	Clear	Neutral	Pale straw
2 p.m.	Up and dressed	Decided trace	"	Acid	Straw
4 p.m.	"	Trace	"	"	Orange
4.30 p.m.	Going to bed	"	"	"	"
6 p.m.	In bed	Shade ³	Turbid	Neutral	Dark orange
8 p.m.	"	Shade ¹	Clear	Acid	Straw
Feb. 16, 6 a.m.	"	None	Turbid	Alk.	Deep orange
10 a.m.	"	"	"	"	"
noon	Getting up	"	Clear	"	Straw
4 p.m.	Going to bed	Trace	"	Acid	Orange
8 p.m.	In bed	Shade ³	"	"	Straw
6 a.m.	"	None	Turbid	"	Dark orange
8 a.m.	"	"	"	Neutral	Orange

TABLE V.—*Showing characters of urine of seven persons who presented 'postural albuminuria' (I to XVII), and of two persons whose urine approached the normal (XVIII to XXII).*

Name and age.	No. of observation.	Time spent erect (E.) or recumbent R.).	Urine.							
			Volume in ounces.	Clear or turbid.	Colour.	Specific gravity.	Reaction.	Albumen.	Triple phosphates.	Urea percentage.
Wm. M., 7 years	I	E., 4 hrs., noon to 4 p.m.	2	Clear	Light straw	...	Acid	Shade ³	None	1.68
		R., 20 hrs., rest of 24 hrs.	16	Slightly turbid	"	...	"	None	"	1.72
	II	E., 4 hrs., 11 a.m. to 3 p.m.	2½	Clear	"	...	"	Shade ³	"	1.9
		R., 20 hrs., rest of 24 hrs.	12	"	"	...	"	None	"	1.92
	III	E., 8 hrs., 10 a.m. to 6 p.m.	8½	Slightly turbid	"	...	Slightly acid	Shade ³	"	1.2
		R., 16 hrs., rest of 24 hrs.	19½	Clear	"	...	"	None	"	1.2
	IV	E., 12 hrs., 8 a.m. to 8 p.m.	13	Slightly turbid	"	...	"	Shade ³	"	2.32
		R., 12 hrs., rest of 24 hrs.	11	"	"	...	"	None	"	2.5
	V	E., 4 hrs., noon to 4 p.m.	3¼	Clear	"	...	Acid	Trace	"	
Richd. B., 15 years		R., 20 hrs., rest of 24 hrs.	46½	"	"	...	"	None	"	2.55
	VI	E., 4 hrs., noon to 4 p.m.	1	"	"	...	"	Shade ³	"	1.15
		R., 20 hrs., rest of 24 hrs.	65¾	"	Orange	...	"	None	"	1.22
	VII	E., 8 hrs., 10 a.m. to 6 p.m.	20¾	Slightly turbid	"	...	"	Shade ³	"	
		R., 16 hrs., rest of 24 hrs.	37½	"	"	...	"	None	"	1.8
	VIII	E., 8 hrs., 10 a.m. to 6 p.m.	16	"	"	...	Neutral	Trace	"	1.66
		R., 16 hrs., rest of 24 hrs.	38	"	"	...	"	None	"	1.35
	IX	E., 8 hrs., 10 a.m. to 6 p.m.	12¼	Clear	"	...	Acid	Trace	"	2.0
		R., 16 hrs., rest of 24 hrs.	20	Slightly turbid	Straw Pale orange	...	Neutral	None	"	2.8

Alice S., 16 years	X	E., 8 hrs., 10 a.m. to 6 p.m. R., 16 hrs., rest of 24 hrs.	$8\frac{1}{2}$ $34\frac{1}{2}$	Clear Turbid	Straw Dark straw	...	Acid Neutral	Trace None	" "	2.16 2.13
	XI	E., 12 hrs., 8 a.m. to 8 p.m. R., 12 hrs., rest of 24 hrs.	$16\frac{1}{4}$ $18\frac{1}{4}$	" Clear	Straw	1031	Acid	Trace	"	
	XII	E., $6\frac{1}{2}$ hrs., 9.30 a.m. to 4 p.m. R., $17\frac{1}{2}$ hrs., rest of 24 hrs.	$17\frac{1}{4}$ $37\frac{1}{2}$	Turbid Clear	" White Straw	1028 1024 1018	" " "	None Trace None	" " "	0.98 1.02
	XIII	E., 4 hrs., noon to 4 p.m. R., 20 hrs., rest of 24 hrs.	6 39	"	Orange	...	"	Trace	"	
Fred. W., 19 years	XIV	E., 8 hrs., 10 a.m. to 6 p.m. R., 16 hrs., rest of 24 hrs.	$39\frac{1}{4}$ $12\frac{1}{4}$	Slightly turbid	"	...	"	Shade ² Shade ³	" Yes	
	XV	E., 8 hrs., 10 a.m. to 6 p.m. R., 16 hrs., rest of 24 hrs.	20 18	"	Dark orange Dark straw	...	"	None	Yes	1.46
	XVI	E., 4 hrs., noon to 4 p.m. R., 20 hrs., rest of 24 hrs.	$17\frac{1}{2}$ 33	" Turbid	" White	1034	Acid	Trace	None	1.2 3.28
	XVII	E., 4 hrs., noon to 4 p.m. R., 20 hrs., rest of 24 hrs.	19 4	"	Pale orange White	1030	Acid	None	"	3.05
Arthur T., 8 years	XVIII	E., 4 hrs., 11 a.m. to 3 p.m. R., 20 hrs., rest of 24 hrs.	$4\frac{1}{2}$ $33\frac{1}{2}$	"	Straw	1025	"	Decided trace	"	0.99
	XIX	E., 8 hrs., 10 a.m. to 6 p.m. R., 16 hrs., rest of 24 hrs.	$25\frac{1}{2}$ 9	"	"	1019	"	Shade ³ None	"	0.96
	XX	E., 4 hrs., 11 a.m. to 3 p.m. R., 20 hrs., rest of 24 hrs.	$16\frac{1}{2}$ 4	"	"	...	"	"	"	2.32
	XXI	E., 8 hrs., 10 a.m. to 6 p.m. R., 16 hrs., rest of 24 hrs.	$37\frac{1}{2}$ $8\frac{1}{2}$	"	"	...	"	"	"	1.95 1.18
Jas. S., 12 years	XXII	E., 12 hrs., 8 a.m. to 8 p.m. R., 12 hrs., rest of 24 hrs.	$22\frac{1}{4}$ 19	"	"	...	"	"	"	1.22
		E., 4 hrs., 11 a.m. to 3 p.m. R., 20 hrs., rest of 24 hrs.	$37\frac{1}{2}$ $8\frac{1}{2}$	"	"	...	"	"	"	2.02
		E., 8 hrs., 10 a.m. to 6 p.m. R., 16 hrs., rest of 24 hrs.	$22\frac{1}{4}$ 19	"	"	...	"	"	"	2.1
		E., 4 hrs., 11 a.m. to 3 p.m. R., 20 hrs., rest of 24 hrs.	$37\frac{1}{2}$ $8\frac{1}{2}$	"	"	...	"	"	"	2.46

Charles R., IX. In such cases the non-detection of albuminuria during the recumbent posture cannot be ascribed to dilution. Neither can the increase of the flow of urine have had to do with the albuminuria in the erect posture: for William M. secreted on occasion almost identically the same volume of urine per hour whether erect or recumbent, e.g. II and III; and Richard B. almost similarly, e.g. VIII; or again Alice S., XI; and yet the same difference was observed as regards the appearance of albumen. Other evidence that the mere amount of urine passed per time-unit had not to do with the albuminuria is seen in the fact that the volume of urine per time-unit was on other occasions diminished while the patient was erect, and yet the same phenomenon occurred, e.g. William M., I, Charles R., X, Richard B., V and VI.

There are many factors concerned in the time-unit volume of urine: it will not, however, be necessary to engage with them here further than to say that all the persons referred to in Table V remained in the ward throughout the period covered by the observations.

2. *Amount of urea, absolute and relative.* Was the urea increased or diminished, while the patient was erect, either absolutely or relatively to the amount of urine? I made several urea estimations in order to answer these questions. Mahomed, as I have said, found the urea largely diminished when his patient was up and about. I estimated the urea by means of Russel and West's apparatus: hypobromite of potash, prepared on each occasion a few minutes before making the estimation, was the reagent used; and albumen, when present in the urine, was first separated by acetic acid: urine containing triple phosphates was not submitted to a urea-estimation.

Before considering the results obtained with persons suffering from postural albuminuria, it may be of interest to show that in two of the more healthy convalescents, who had been quite free of albuminuria for several weeks, the urea varied but little with posture, though it did vary with other circumstances. For instance, James S. on two occasions (XXI and XXII) excreted much the same percentages and not widely different total amounts of urea per hour while up, and while in bed. Similarly with Arthur T. at XIX. Yet on another occasion the former excreted while erect a slightly diminished percentage and a considerably diminished absolute amount of urea, as at XX; and the latter excreted while

erect an increased percentage and a reduced absolute amount of urea, as at XVIII. In these two instances (XX and XVIII), however, the period spent in the erect posture was from 11 a.m. to 3 p.m. and included the period of active digestion after dinner, during which for two hours the urine is diminished, as was found by M. Chossat: whereas in the two former instances the period spent in the erect posture was more prolonged, so that time was afforded for the redistribution of the fluids of the body, after they had been largely draughted off into the chylopoietic viscera during the process of digestion.

I now turn to the cases of postural albuminuria. Concerning them it may be noted that Richard B. on one occasion (VIII) passed almost the same percentage amount and almost the same total of urea per hour, whether up and about or recumbent. William M. also passed almost identically the same percentage amount, and the same total of urea per hour on two occasions (II and III), and again not very dissimilar amounts, relative and absolute, as at IV; and similarly with Fred. W., as at XV.

It may be mentioned that in the observation numbered XIII a little albumen continued to be passed with the urine after the patient had lain down: but taking all the albuminous urine (of ten hours) together, viz. 21 ozs., I found that it yielded 1.95 per cent. of urea, while all the non-albuminous urine of the remaining 14 hours, viz. 24 $\frac{2}{3}$ ozs., yielded 1.75 per cent. of urea.

Hence, in the cases of postural albuminuria, the percentage amount and the absolute amount of urea stood in no definite relation to the presence or absence of albumen. They might be almost precisely the same, whether the patient were erect or recumbent; and yet albuminuria appeared only during the erect posture. In other instances the urea was increased, and in others again it was diminished, both relatively and absolutely; and yet the same sequence of events as regards albuminuria and posture was observed. It may, then, be concluded that the appearance of albumen in the urine did not depend upon either the absolute or the relative amount of urea separated. The case of William M. alone, in fact, suffices to show that this was so; as also it serves to show that the presence or absence of albumen was not dependent upon the total quantity of fluid separated: a conjunction of circumstances in the same individual which may be laid stress upon.

There were also several cases in which the percentage amount of

urea was all but the same while the patient was up or in bed ; and yet albumen appeared only while the patient was up : for instance, Alice S., XII, Louisa L., XVI, Emily F., XVII, William M., I, Charles R., X. Again, the non-albuminous urine passed during recumbency by Richard B. on one occasion, at VII, was diminished and contained a larger percentage of urea than that passed while erect ; whereas, on another occasion, at VI, it was much increased and the urea was relatively much diminished.

It may be noted that, if the albuminuria of purely postural cases be due to a slight glomerular affection, the amount of urea would not be expected to bear any definite relation to the presence or absence of albumen in the urine.

3. *Other urinary solids.* These I did not find time for estimating. I may say, however, that, with change of posture, the specific gravity remained the same, decreased or increased ; and yet the albumen appeared only when the patient took the erect posture. The absolute and relative amounts of indican, roughly estimated, bore no definite relation to the appearance or disappearance of albumen. In so far as colour may serve as an indication of the composition of the urine, it deserves notice that, as shown at III and less clearly at IV, the urine might be very similar in volume per hour, very similar in its percentage amount of urea, and the same in colour (a light straw), whether the patient were erect or in bed, whether the urine were or were not albuminous.

4. *The reaction of the urine.* Table V shows that the urine was acid in most instances, whether the patient were recumbent or erect : the reaction might be as nearly identical as it was possible to ascertain by litmus-solution ; and yet the albuminuria came on only when the patient was erect. As to the absolute amount of acid, of course, where the reaction was identical, and the amount of the urine differed, in any two cases, the amount of acid would be larger in that urine which measured most ; still, on making reference to William M., III and IV, it will be seen that the amount of acid, whether relative or absolute, had nothing to do with the albuminuria. It will be seen, from Table U, that a patient, whether erect or recumbent, might excrete neutral urine (e.g. Charles R., February 16), or acid urine (e.g. William M.), and yet the albuminuria appear only when the patient was erect. The same may be seen in Table V (at VIII, XI, XIII, XIV). The urine might,

moreover, be acid or neutral while the patient remained in bed, and then neutral or acid respectively when the patient got up, and yet the albuminuria appeared only on taking the erect posture.

5. *Time of day.* The albuminuria came on in the erect posture, it seemed, as readily by night as by day, as readily at one part of the day or night as another; and as readily whether the patient got up once or oftener in the course of the twenty-four hours: the amount of albumen passed differing, however, somewhat. Some data specially bearing upon this subject are given in Table U.

For instance, William M. on February 12th passed as usual no albumen while in bed, i.e. till 9 a.m.: he then got up; albumen was detected at 10 a.m.; it continued to be present till 8 p.m., i.e. for two hours after getting into bed; and it was not found in the urine passed afterwards while he remained in bed. On the previous day he got up later, viz. at 10.30 a.m., and albumen had not been detected before that time; it was found soon after he got up; but it disappeared after 12 noon on returning to bed. On the following day he got up and went to bed twice; and albumen appeared shortly after getting up and disappeared shortly after getting to bed on each occasion. The same was true of the other patients examined.

6. *Ingestion and digestion of food.* Four meals were taken by the patients referred to in Tables U and V, as follow: breakfast at 8 a.m., dinner at 1 p.m., tea at 4 p.m., supper at 8 p.m. As may be seen from Table U, William M. on February 12th was out of bed during dinner and tea, and he then had albuminuria; whereas on the previous and on the following day he was in bed during and after dinner, but on neither occasion did he exhibit albuminuria. It was the same in other postural cases.

7. *Nature of food.* As already said, postural albuminuria appeared when the person was erect whether the diet were milk, fish or meat; and recovery, indeed, was, I think, more rapid when a little meat or fish was taken. [It may be of interest to remark here that iodide of iron was of value, and especially when administered along with iodide of potassium: hypophosphites did not seem to have any effect: oxygen inhalations were, I think, of value.]

8. Several authors have shown that disturbances of the nervous system may cause albuminuria, e.g. M. Solon, C. Allbutt, Fürbringer, Laycock, Sir A. Clark. Fürbringer relates that mental excitement with depression always caused albuminuria in a medical man, a

patient of his, though this symptom was never manifested after long and fatiguing work, after meals rich in albumen, or after free use of alcohol: and Max Huppert found albuminuria associated with mere epileptic vertigo.

Mental influence seemed, however, to have nothing whatever to do with albuminuria in my postural cases.

9. *Physical causes.* Putting aside the several factors already considered, there are others more essentially mechanical, which may be thought of as having had to do with albuminuria of the erect posture. These are numerous no doubt; but of them may be mentioned, as more worthy of consideration:—the venous column above the renal veins; such changes of the intra-renal vessels as might be due to incoordinate action of vasomotor nerves; acceleration of the heart and respirations. Such mechanical causes are the more readily entertained, if the very remarkable rapidity be considered with which albumen (not detectable by the most subtle of tests in urine passed when the patient is recumbent) appeared when the patient got up, and that with which it disappeared when the patient lay down. The following facts may be quoted in illustration. The lad Charles R., while in bed at 12 noon on one occasion, passed urine, which gave not the faintest turbidity with picric acid: he thereupon got up and dressed, and the urine passed at 12.10 p.m. reacted faintly with the acid; this reaction was more marked in the urine passed at 12.20 p.m.; and the urine passed at 12.30 p.m. showed a marked shade of turbidity on the addition of the acid. Again, William M. got up one day at 11 a.m.; at 2 p.m. he passed $\frac{1}{2}$ oz. of urine which became turbid with the acid; at 3 p.m. he got into bed, but first passed $\frac{1}{2}$ oz. of urine, which deposited a flocculent precipitate with picric acid; at 3.30 p.m. he passed $\frac{1}{4}$ oz. of urine, which yielded a very faint opacity with the acid; and at 4 p.m. he passed $1\frac{1}{4}$ ozs. of urine, which yielded no such reaction. On another occasion at 8 a.m. while in bed he passed urine, which gave with picric acid no turbidity whatever; at 8.15 a.m. just before getting up the same occurred; at 8.30 a.m. his urine became slightly turbid with the acid; and at 8.45 a.m. another specimen yielded a distinct reaction. It, therefore, appears that ten minutes spent in the erect posture sufficed for albumen to make its appearance in the urine; a fact, which would of itself suggest a mechanical cause. I may add that commonly the albumen was increased by exercise; and that in

some cases it diminished somewhat towards evening after the patient had been up for some hours.

Before, however, entering upon a detailed consideration of the mechanical factors possibly operative in bringing about this postural albuminuria, it may be of advantage to refer to the similar, if not identical, phenomena that have been described by various authors. My cases appear to have agreed in all essential respects with those spoken of by B. Jones, Bartels, G. Johnson, and Mahomed: but it has been said that albuminuria may be intermittent in persons who present no other evidence implicating or suggesting mischief in the kidneys, which indeed are supposed to be healthy. The latter form of albuminuria has been variously designated intermittent, remittent, adolescent, cyclic, paroxysmal, latent, functional; and it will be of advantage to review what has been said concerning it.

Bright himself observed in 1827 that albuminuria could be detected in the great majority of persons in apparent health; and he said of such albuminuria in his own person that it was increased to a considerable amount by the slightest causes. Sir W. W. Gull in 1873 spoke of the urine of young and growing men as being frequently albuminous, though otherwise normal; and he ascribed the symptom to atony of the renal vessels and nerves. Leube in 1878 found that albuminuria came on with exercise, and that it disappeared within a few hours afterwards, in 12 per cent. of 119 apparently healthy soldiers. Moxon, in the same year, spoke of albuminuria being at times intermittent or again remittent, the non-albuminuric periods occurring at considerable intervals or in some cases even every day, generally during the evening and night, the albuminuria being most pronounced after breakfast: he suggested that irritation of the renal tissue by morbid matters (e.g. oxalate of lime, commonly found in the urine in these cases), was the cause of the albuminuria; or, again, that the intermittent form, occurring as he thought in particular among adolescent males¹, was related to puberty, and that it might be due to a condition of kidney correlated with excessive activity of the testes,—for a special developmental correlation of structure between the testis and kidney had been observed by Rokitsansky. Dr. T. M. Rooke, Dr. B. Yeo, Dr. C. Dukes and Dr. Ferguson, also, in the same year

¹ Several writers say that this so-called albuminuria of adolescents may be due to secret impurity.

published cases of similar kind. In three anaemic girls (fourteen to sixteen years of age) Dr. Rooke found albuminuria, which quickly disappeared on lying down. Dr. Yeo spoke of a man whose albuminuria was specially related, it seems, either to the assumption of the erect posture or to exercise. And Dr. Dukes found this affection commonly among the boys under his medical care at Rugby; he ascribed it to various causes,—sudden change of temperature, error of diet, excessive exertion, emotion; and, seeing the frequency with which it occurred at the time of puberty, he thought the increased vascular tension, said by Beneke to occur at that time, was a predisposing cause, ‘this increased forcing of the blood often’ rendering ‘the kidneys hyperaemic,’ and so specially liable to yield to trivial causes. He found that it might disappear after a few days, and recur with every subsequent chill or error of diet for months, or that it might be very persistent. It might cease at once on confinement to bed between blankets, and reappear on getting up; and, whereas while in bed the taking of full meat diet had no effect in evincing this symptom, it had a determining effect this way when the person was up and about, the mode of differentiation as between influence of posture and that of food not being, however, stated. Posture was evidently of marked importance. Towards evening the albuminuria became less marked. Dr. Ferguson’s patient, whose urine though not albuminous in the early morning became albuminous later on in the day, had, it seems, renal disease; puffiness of the eyelids was in this case the first symptom that directed attention to the urine. In 1879, Dr. Quain referred to a patient, whose albuminuria was considerable after breakfast though it subsided in the course of the day. In 1884 the subject was again specially referred to by Dr. Pavy. Having observed that in several males, of ages 9 to 49 years, albuminuria came on in the morning after breakfast, and that after reaching a maximum of intensity it diminished, so as often wholly to disappear towards night; he classed these phenomena with the diurnal cycles pointed out by Dr. Bence Jones and Dr. Edward Smith, and proposed the epithet ‘cyclic’ for this form of albuminuria. This term has been objected to because the appearance of the albumen can be, as had been previously observed and as was very shortly afterwards again shown in a ‘Lancet’ leader, prevented or determined according as the person lay at rest or got up,

one person (a female) quoted in the 'Lancet' having passed albumen 'shortly after rising, no matter whether she rose morning, midday or afternoon.'

It may be observed, however, that other cyclic manifestations may similarly be rearranged, as I showed in the 'British Medical Journal' of July 26th, 1884, in connexion with the body-temperature.

Dr. Pavy found the urine normal, save for the presence of albumen and crystals of oxalate of lime: and he found no impairment of health and no constitutional indication of Bright's disease; the heart's impulse might be sharp and unduly forcible, but it was not heaving and the pulse was soft. In a second communication Dr. Pavy spoke of three persons (11 to 20 years of age, one a female), who, though exhibiting albuminuria after being erect a short time, ceased to do so soon after taking the recumbent posture, and this independently of meals. To the mother of one of them it appeared 'as though getting up caused the albumen to filter through the kidney;' she had noticed that 'the albumen did not appear till the boy had been on his feet for about an hour.' Dr. C. von Noorden in 1886 confirmed the observations previously made; having found the affection oftenest in youths between puberty and twenty years of age, the albuminuria being at a maximum in the forenoon and rarely continuous all day: he found no other evidence implicating the kidneys and generally no constitutional abnormality. Stewart Lockie, again, while agreeing with others as to the direct influence of posture, thought that the principal factor was gout, either then in evidence or latent, each of his four patients (one a female) being either gouty or of a gouty family. Another patient, a female aged sixteen years, under the care of Dr. Biss, was closely observed and reported upon by Dr. S. Coupland, who examined the urine every four hours or so for twenty-three days. She complained only of boils in the neck, and pains in the back and legs: the cardiac sounds were abnormally loud and booming; the second sound was occasionally reduplicated; and there was no evidence of enlargement of the heart. While under observation she got up at 5 a.m., returned to bed at 8 a.m. (7 a.m.?), got up a second time at 11.30 a.m., and retired for the night at 8 p.m. The urine passed on different days at 4 a.m. never contained any albumen; on only two occasions was albumen found in the urine passed at midnight, on one of these owing no doubt to

the fact that the urine had not been passed at 8 p.m. It was at 8 p.m., i.e. 'at the close of a day of comparative activity' that albumen was most frequently found; and at 8 a.m. only somewhat less frequently. Diet, and time of taking food, had practically no influence; and there was no observable relation between the appearance of albumen and specific gravity or colour of the urine. Indeed, erect posture and exercise were the chief factors concerned. The urine had a specific gravity of 1026 to 1036; it was always acid, generally clear; it did not contain sugar, peptones or haemoglobin, nor did it deposit any casts, uric acid or oxalates. In 1886 and 1887 Dr. C. Ralfe drew special attention to an intermittent form of albuminuria independent of renal lesion, the urine containing albumen, excess of urea and of urobilin, intermittently; and he supposed that it was essentially of the same nature as paroxysmal haemoglobinuria, both being attributed by him to excessive haemolytic action in the liver. M. Teissier, also, spoke of ten persons, in whom albuminuria was directly excited by violent exercise and excessive emotional excitement; and he ascribed it to over-action of the liver; the urine contained bright metal-like particles and oily matters. Dr. Merley, who has recently confirmed these observations, thinks that the primary fault is insufficiency of general metabolism, and that the affection is allied to gout. In 1887 Dr. A. W. Stirling reported the occurrence of 'postural albuminuria' in 77 out of 369 boys on a Thames training ship; the albuminuria came on at 9 a.m., about three hours after rising: and so assured was he of its direct relation to the erect posture, and to it alone, that he adopted for it the above designation. The albumen appeared within a short time of getting up, independently of time of day when the boy got up, independently also of food and all other conditions save the erect posture; it continued to appear for one or two hours; and then it generally diminished, so that, if the boy got up in the morning, it was absent by bedtime, though in some instances it persisted throughout the day; and it disappeared on lying down at any time. It might increase after a meal, but only to a trivial extent: whether relatively or absolutely is not stated. Exercise by walking had no effect, neither had the manipulation of a heavy weight for twenty minutes while recumbent in bed. He ascribed the albuminuria in these cases to hyperaemia of the kidney; 'jumping out of bed causing a sudden rush of blood to,

and a dilatation of, the renal vessels,' the latter requiring then some time to recover from the shock. He found no evidence of increased haemolysis, and none of the fatty matters or spangles described by Teissier and Merley. [These bodies were not observed in any of my cases.] The arterial tension was generally low; the heart was only rarely enlarged or its impulse accentuated; and the pulse was often soft. He alluded, also, to a man, who, though all along in fairly good health, had manifested this form of albuminuria for six years, and as markedly then as at any previous time: the retina in this case was perfectly healthy, and the pulse soft.

As I have said above, it has been supposed that albuminuria, intermitting in the way shown in these examples, is independent of any renal or transrenal change of structure, is purely what I have called *eisrenal*; and such cases have been put together as a class entitled *functional, latent, potential*. Some of the cases above quoted may without doubt be correctly spoken of as not independent of renal lesion. Others, such as the paroxysmal form described by Ralfe, may be thus independent. Others, again, may be due to temporary irritation of the renal tissues,—such as has been insisted upon by Dr. George Johnson ever since 1852, owing, for instance, to excretion of glucose or of bile. There remain, however, other cases in which albuminuria appears whenever the person gets up, and disappears whenever the same person lies down. Are these due to a mere temporary renal irritation, an irritation independent of any prior renal mischief? There seems to be no reason whatever for admitting this as a reasonable explanation; they must be regarded as cases in which more essentially mechanical factors are directly concerned. Of the various forms of intermittent albuminuria there is in fact a class, of which the immediately exciting cause is the assumption and maintenance of the erect posture:—ingestion of food, excessive eating, excessive exercise, abnormal elimination of bile pigment, of urea, of glucose in the urine, hepatic disorders, mental influences, and such mechanical factors as the irritation of the urinary passages by crystals or the pressure occasioned by splenic and other tumours, with all such influences aside, there remains the erect posture as the prominent influence in the escape of albumen into the urine of a large number of persons, of various ages, of either sex; the albuminuria subsiding after a few hours while the person remains erect, or very quickly after lying

down ; the urine being normal, it is said, in other respects, save for the presence in some cases of crystals of oxalate of lime.

The dependence of albuminuria, which is not transrenal, upon mechanical factors would, however, indicate either some defect in the kidneys or something unusual in the blood-pressure, or both. The patient referred to by Dr. Bence Jones, and whose urine could be made albuminous or non-albuminous according as the circulation was 'hurried by motion or quieted by rest,' was suffering from Chyluria ; and it is known that, though there may be no evident structural change in the kidney in this affection, the blood-vessels of the kidney contain the filaria and their ova in considerable numbers. Mahomed's patient had had scarlatinal nephritis ; and the persons referred to by Bartels were suffering from renal cirrhosis. It is, also, important to note that each of these observers referred the albuminuria to an essentially mechanical cause. Dr. Johnson, in speaking of this form of albuminuria, was alluding to persons recovering from acute nephritis : and, as regards my own patients, each of them was recovering from scarlatinal nephritis, I have supposed from glomerulitis and changes in the convoluted tubes.

It will, therefore, be desirable to consider more critically, from this point of view, those of the above described cases which have by some been regarded as independent of renal change. First, however, I would quote the weighty authority of Dr. Johnson. In the 'British Medical Journal' of 1879, he expressed his opinion that albuminuria, without other evidence of functional disorder or structural disease, might generally be traced back to acute nephritis with or without dropsy, such as arises from exposure to wet or cold, from an attack of Scarlatina, Measles, Diphtheria, Erysipelas, Typhus or Typhoid fever, Pyæmia, Rheumatic fever, or from pregnancy, excess of animal food, alcoholic stimulants, mental anxiety ; and that albuminuria of boys was, likely enough, the result of reckless exposure to cold and wet, especially after violent exercise. Let us now turn to the histories, in so far as they have been put on record, of the patients referred to as having exhibited intermittent albuminuria.

Of the nine patients, whose cases were detailed by Dr. C. Dukes in 1878, three had suffered from scarlatina ; another from tonsillitis with acute hæmorrhagic nephritis ; another from a slight

cold, while lying in a bed next to one occupied by a boy who had suffered from scarlatina; another had had a slight cold; another at the outset had rigors and headache; and another was out of sorts, and had a hard slow pulse. Of the seventy-seven boys on the training ship mentioned by Dr. Stirling, most had suffered from measles; but a history of scarlet fever or of diphtheria had not been at all common among them. Dr. Stirling also found that among the band-boys there was a larger proportion of cases of albuminuria than among the other boys. The question may, then, be asked whether these earlier illnesses, the playing on wind-instruments, perhaps bathing (which, however, is not spoken of), had caused any renal defect. Moxon, though excluding ordinarily accepted cases of albuminuria, did not regard his cases as physiological. One of Dr. Pavy's second series of three patients was supposed to have (recently?) had a mild attack of scarlatina. Of Teissier's cases, three had had eczema or urticaria. The patient, under the care of Dr. Biss, suffered from boils. One of Dr. Lockie's four patients had follicular sore throat when the albuminuria was detected; and in all four there was either a gouty predisposition or actual manifestation. And the patient, recently recorded by Prof. G. Stewart, was at the time convalescing from diphtheria. Prof. Stewart supposed that there was no renal disease in this patient, and for the following reasons:—albuminuria had not been detected before the patient began to get up; it occurred for only a portion of each day; the amount of urine and of urea was normal; and casts were seen on only two occasions: because, in fact, there was no symptom except albuminuria to suggest it.

Hence, even in the purely postural cases, the histories are strongly suggestive of the existence of renal mischief; indeed, a large proportion of them lend support to the opinion expressed in 1879 by Dr. Johnson. There can, too, be no doubt that we have yet much to learn of the possible causes of nephritis, especially, perhaps, of mild forms of glomerulo-nephritis. Dr. Johnson finds that the latter may result from inhalation of sewer gas. Limited primary alveolitis, of a very mild sort, certainly exists in a large proportion of persons; it is likely that the same is true of mild and perhaps limited glomerulitis: the great persistence of both these affections may be partly the result of the constancy of the to-and-fro movement maintained in alveoli and glomeruli alike.

Several authors have laid great stress upon the character of the pulse in postural albuminuria, as indicating that there is no renal disease, assuming that accentuation of the second cardiac sound, reduplication of that sound, a heaving character of the impulse, are necessarily present if there be nephritis or renal cirrhosis. In this connexion I may point to Mahomed's statements as to the absence of rise of tension in some cases of acute haemorrhagic nephritis; and I may add that in a large number of my own patients, who certainly had renal inflammation, there was no perceptible rise of tension.

What part, then, may we infer cardiovascular phenomena should play in mild cases of renal glomerulitis or tubulitis? Likely enough no appreciable part whatever.

I have, however, supposed that postural albuminuria is not the expression of any active progressive change in the kidneys. It has been found (though, I may repeat, not in all cases with clear differentiation of the influence of posture from that of exercise), as already remarked, in several affections; in chyluria, in cirrhosis, and in late stages of diffuse inflammation of the kidneys, all of which have at least this in common, that the vessels of the kidney either are not normal or are in all probability subjected to abnormal strain when there are even slight alterations of blood-pressure.

It is immaterial to contend that cirrhosis and inflammation of the kidney are to the 'dualist' different affections; for it has been found that in cirrhosis the glomeruli are perhaps as frequently implicated as in inflammation of the kidney. Prof. J. Fischl, in 1884, asserted that he had found the Malpighian bodies affected in 90 per cent. of cirrhotic kidneys; MM. Cornil and Brault, also, who regard cirrhosis as a non-inflammatory affection, find the glomeruli very generally affected. Dr. Waller, indeed, as I have said, thinks that cirrhosis commences in the same way as diffuse inflammation of the kidneys; and both Dr. Dickinson and Dr. George Johnson have recorded cases of granular kidney, the origin of which was traceable to scarlatinal nephritis. Rosenstein, also, looked upon the small red kidney as resulting from diffuse inflammation. Some have, it is true, questioned the likelihood of two such different conditions resulting from one process; but there are facts which strongly support the view that they do. Aufrecht, for instance, states that whereas large doses of cantharidin quickly cause acute nephritis in animals and death, smaller doses cause forms of nephritis which differ from typical acute nephritis, and very small doses repeated over a period of four months result in the formation of the granular kidney. MM. Cornil and Brault, also, suppose that the results, obtained in kidneys by injections of cantharidin into the system, vary with the quality of the cantharidin, the dose, mode of administration, frequency and regularity of repetition of the dose, and the idiosyncrasy of the animal injected: they find that small doses act much less generally on the renal structures than large doses; and that, if frequently repeated, they may affect certain parts to the exclusion of others.

Among my own patients it is quite likely that by changes in the earlier stages there had been brought about a condition of the walls of the tubules and of the glomerular tufts, which allowed some slight mechanical factor to cause the escape of albumen.

We may now proceed to consider the mode in which mechanical factors may be supposed to act.

The immediately determining cause, likely to be first suggested, and indeed that which has generally been advocated, is mechanical hyperaemia. The latter, when the kidneys are healthy, causes slowing of the renal blood-current, engorgement of the renal veins and capillaries, after a time compression of the renal arterioles, and the opening up of collateral routes for the blood shorter than those routes *via* the more superficial of the glomeruli. The urine is diminished, its specific gravity and depth of colour are increased; and it receives albumen primarily (as shown in 1843 by Robinson, and afterwards by Frerichs) from the tubules, and secondarily (as shown by Senator) from the glomeruli.

In some of my cases these same changes were observed when the patient took the erect posture. As further serving to show that mechanical hyperaemia has the direct effect of promptly causing albuminuria in persons who have but just convalesced from the nephritis of Scarlatina, I may refer to patients who, after having lost all trace of albuminuria, contracted whooping cough, pneumonia, capillary bronchitis, or other disorder involving respiratory embarrassment, and very quickly afterwards again manifested albuminuria, which I came to regard as secondary, as the result, in fact, of obstruction to the pulmonary circulation.

Moreover, there were several patients in whom, though the assumption of the erect posture had the almost immediate result of causing albuminuria, the latter symptom did not very rapidly disappear on lying down: and in some of these cases the urine contained a great profusion of cells from the straight and collecting tubes. I supposed that venous hyperaemia accounted for both the albuminuria and the shedding of cells, the latter as a result of compression of the tubules owing to engorgement of the inter-tubular vessels.

In the purely postural cases, however, the albuminous urine contained scarcely any sediment (at most but a few leucocytes); and its volume was not only not diminished, but in some instances was

increased (e. g. Table V, obsn. VII, IX, XIV, XV). In cases in which the urine was increased, either the inter-tubular plexuses and the glomerular vessels allowed a more free passage to the blood, or the tubules or glomeruli allowed transudation to occur more readily, when the patient was erect. It would, therefore, be at least doubtful in these cases whether venous hyperæmia was the sole or chief factor directly concerned in the escape of albumen. It would appear, also, to be improbable that it had direct concern in bringing about albuminuria in the patients mentioned by Bartels, patients in whom the heart may be presumed to have been acting well (*vide* Bartels, *l. c.* p. 396). The like doubt may, also, be raised in connexion with the case mentioned by Bence Jones. And it is to be remarked that both these observers referred the albuminuria to causes operating, not from the venous, but from the arterial, side of the renal circulation. Bartels, indeed, supposed that the albuminuria in cirrhosis was always, and solely, due to increase of blood-pressure in the glomeruli.

Consideration may, then, be given to the arterial blood-pressure in the kidneys. In this connexion it has occurred to me that more importance may attach to *alternations* of this pressure than has hitherto been allowed.

The healthy kidney expands during inspiration and during the cardiac systole; and it recoils during expiration and the diastole, as may be shown by means of the oncograph. The alternate expansions and contractions of the kidney, primary or respiratory and secondary or cardiac, are due to alternate changes in the amount of its fluid contents, in chief part blood: and doubtless there is, during cardiac diastole and expiration, a reduction of pressure within the Bowman capsules, slight though it be, yet sufficient to materially affect the glomerular transudation.

Many authors have thought that in the renal glomeruli, at least of Mammals, there is a considerable retardation of the flow of blood, and a considerable increase of blood-pressure. Bowman supposed it to be so, and Mahomed thought there must be even pulsation in the glomerular vessels. In animals below Mammals the slowing of the blood current, and the amount of blood-pressure are no doubt less: the glomeruli in some of them (Reptiles and Birds) are but short coiled dilatations of the afferent arterioles; the efferent vessel may be as large as the afferent; and the efferent vessel does not pass out from its Bowman capsule with the afferent vessel, nor from between any primary divisions of the afferent vessel within the glomerulus; and we may, therefore, suppose that retardation of the stream, though probably occurring, is not so great as in Mammals, and that the

blood-pressure is not so high. If, then, there be repletion of the tufts with systole and with inspiration, and some depletion with diastole and expiration, as would seem to be indicated by the tracings of the oncograph, we may suppose that in animals with vigorous cardiac and respiratory activity there are considerable alternations of intra-capsular pressure, i. e. unless the kidney as a whole and each capsule individually follow accurately the to-and-fro movements of the tufts. Mammals and Birds, however, stand quite apart from Vertebrates beneath them owing to the activity of their respiratory and cardiac movements, despite the condition of diaphragm in the bird. On the other hand, these latter Vertebrates having less respiratory suction power, less powerful cardiac action, and perhaps less compactness of their kidneys, are possessed of a mechanism which, as far as urination is concerned, may take their place; I mean ciliary movement in the renal tubules. Bowman was the first to speak of cilia in the renal tubules: he found them in 'the frog, other reptiles as well as fishes,' and noted their action in reducing pressure from the surface of the glomeruli. I would ask whether the ciliary suction exercised in lower animals is replaced or enhanced¹ in higher animals by glomerular pumping: the cilia, less required in the latter owing to their more powerful circulatory mechanisms, having in a phylogenetic sense become reduced; as the cilia, say, of the digestive tract, have become atrophied in the higher animals, though, it may be added, they persist in the respiratory tract owing, perhaps, to their use in reducing the air-tension in the cavities of the pulmonary vesicles, and in thus facilitating the separation of carbonic acid.

Granting that there is an action of this sort in health, I would next ask whether in kidneys, the resiliency of which has been reduced, throughout or in part, this glomerular pumping is intensified. Is it likely that this is so in the cirrhotic kidney, and that the excess of urine separated by such a kidney is thus brought about; and that in this manner the urine is still further increased, and therewith also the albumen in it, when the heart's action is at any time rendered more vigorous? Is it likely that such want of resiliency is a part-cause of the increase of urine in persons suffering from waxy kidney? If it be so, then with smaller degrees of reduction of resiliency, say as results of slight diffuse inflammation, there will also be some intensification of this pumping, and with it likelihood of escape of albumen. Of course, in order that the volume of urine be increased, it is necessary that there shall be no undue delay in the flow of blood (as occurs in inflammation), and no great amount of obstruction in the glomerulo-tubular systems, as occurs very frequently in inflammation, and perhaps generally in secondary atrophy of the kidney.

It is also to be borne in mind that in cases, in which there is glomerular affection (c.g. in diffuse nephritis, in cirrhosis), ad-

¹ Cilia have been observed in some herbivorous and some hibernating Mammals, e.g. the sheep, horse, rabbit and mouse.

hesions may form between the glomeruli and the Bowman capsules; and that any sudden variation of tension might tear such adhesions and so add to the likelihood of the escape of albumen.

Thirst and absence of dropsy in the person suffering from cirrhosis would thus come to mean excessive glomerular pumping, enhanced by the vigorous action of the heart. And the increase of urine observed as acute inflammation of the kidney subsides would mean—not absorption of urea or of other diuretic agents from dropsical fluids (for it may at least be questioned whether these diuretics would not already have so thoroughly saturated the tissues of the body as to have lost, at any rate for a time, their normal diuretic properties), not any improvement in the action of the heart (certainly no increase of force in its beats), but the return of the glomeruli (the tubules also) to their more natural condition, and therewith the coming into operation of glomerular pumping, which of itself necessitates the removal of all spare fluids just as it also induces thirst.

Let us now revert to the cases of postural albuminuria, and first to those mentioned by Bartels. Bartels ascribed¹ the albuminuria in his patients—affected with cirrhosis—to increase of arterial pressure, which he said was indicated by the fact that the heart's beats were accelerated in one patient from 72 and 76 per minute while recumbent to 90 and 96 while erect. Acceleration of the pulse, however, generally coincides with, and is attributed to, reduction of peripheral resistance in the circulation. On taking the erect posture and moving about, there occurs a large determination of blood to the muscles: and there is reason for thinking, it is said, that during muscular exercise there is diminution of flow of blood through the renal glomeruli; for, Ranke finds that during tetanus the glands contain less blood than when the muscles are at rest,

¹ Referring to a person, the subject of renal cirrhosis, who for four months had albuminuria while erect, and not while recumbent, Bartels said: 'Die ganz constante Erscheinung, dass der Kranke ausser Bette bei freier Körperbewegung Eiweiss mit dem Urin entleerte, niemals aber während der Bettruhe, lässt, wie mir scheint, nur die Deutung zu, dass der arterielle Blutdruck im gesammten Aortensystem und folglich auch in den Nierengefässen durch die Körperbewegung noch mehr über die Norm gesteigert wurde, als es schon in der Ruhe der Fall war. Damit stimmt denn auch der fernere Umstand überein, dass die Pulsfrequenz während der Bettruhe stets beträchtlich geringer gefunden wurde, als bei aufrechter Körperstellung, im Bette zwischen 72 und 76, ausser Bette stehend zwischen 90 und 96, zuweilen bis zu 100 Schlägen in der Minute.'

and there is admittedly during exercise considerable reduction of the volume of urine passed. If this be so, then, if the kidneys have lost in their resiliency, it is likely that the Bowman capsules would not accommodate themselves to the change of volume of the glomerular vessels as they would in health; and the acceleration of the respiratory and cardiac movements accompanying the assumption of the erect posture and exercise would, under these circumstances, tell with redoubled effect upon the glomerular transudation, and so, likely enough, cause the escape of albumen,—especially if the walls of the glomerular vessels had become weakened by disease.

I have thought that some such interpretation as that here suggested by way of explanation of the cases mentioned by Bartels may be applicable to some of my own cases, and to the similar cases which have been described by others; the mechanical changes attendant upon the assumption of the erect posture being of much the same nature as those attendant upon exercise.

It was said, however, by Dr. Stirling that albuminuria did not come on in his postural cases when exercise was taken by the patient while recumbent. Exercise, however, during recumbency does not probably amount to the same thing as exercise while erect: there is not the same free play of the thoracic movements, ‘splinted down’ as the ribs are to the bed or the couch by the weight of the chest itself; and the interference thus brought about to the return of blood from the kidneys to the heart may prevent any unusual degree of glomerular pumping. Moreover, during exercise while erect there is probably a diminution of size of the renal vessels, compensatory to the enlargement of the vessels in the muscles and in the skin attendant thereupon: whereas during exercise while recumbent it is quite likely that this coordinated action does not come into play. Still, Litten, it may be remarked, finds that albuminuria does appear in the dog when fastened down, i. e. it appears as a result no doubt of the struggles of the animal in its endeavour to get free; but in such a case the question remains whether the albuminuria is not venous.

Again, it has been said that albuminuria, when postural, may disappear towards evening or night: this was observed in some of my patients. Why is this? In my patients, in whom this occurred, the amount of albumen passed in the early part of the day was very small: and I supposed that the reduction of albumen towards

evening was to be accounted for as follows. Among the many cases of glomerular lesion which I detected, there were doubtless, especially in their progress towards recovery, degrees of every sort. At later stages of some there would be but slight glomerular inflammation remaining. In such cases, while the patient was recumbent and at rest and the glomeruli were thus but little disturbed by the cardiac and respiratory movements, the Bowman capsules belonging to still slightly inflamed glomeruli might retain the albuminous exudate poured into them: and when the patient got up, these glomeruli, like pulmonary alveoli in a similar condition, would be disturbed and the contents of the corresponding capsules would be somewhat suddenly dislodged; then, though slight loss of albumen might continue through the day, the albumen would pass on as fast as it escaped from the glomeruli, and thus be so far diluted as not to manifest itself in the urine even in the presence of very delicate reagents.

Another factor also operates later in the day. In health there is a general fall of blood-pressure after a person has been up for a certain length of time; and it is possible that this fall affects the renal as well as other vessels. At least the pulse is softer in the evening; and the radial artery has been found by Vierordt, Aberle and Basch to be actually enlarged towards evening to twice the diameter that it has in the morning. Hence, we may suppose that the glomerular tufts become more full, and that glomerular pumping thus becomes less active, towards evening: the volume of the urine passed may be, and probably is, increased: but the alternations of pressure under which the urine is transuded at the glomeruli are then smaller; the outer surface of the tuft is better supported against the wall of its capsule, and albumen is less likely to appear in the urine. If the patient gets up late in the day, albuminuria appears as usual; but, as observed by Dr. Stirling, it is not then so marked, and it more quickly passes off. After a night's rest there is a restored capacity for developing energy, revealed in an improvement of the heart's action and of the vascular tone: as the day passes by, the central and the peripheral vasomotor centres, the muscle of the heart and of the arterioles, in common with the nervous and muscular systems generally, become wearied; the blood-paths are more patent, and the heart beats more frequently though it be less forcibly; the capillary circulation is more free throughout

the body, and there is slight rise of peripheral body-temperature. [In cold weather, this evening reaction is more marked.] In the person with damaged renal glomeruli, albuminuria concurs with the improved vascular tone of the early part of the day, and it disappears as this is on the wane: the diurnal cyclic manifestation of albuminuria, as also other diurnal cyclic manifestations, are, I would suggest, thus brought about. Let the glomeruli be more damaged, then the albuminuria will persist while the person is erect; let them be still further damaged, and the albuminuria will persist night and day, though even then it will be less marked when the patient is kept warm and recumbent in bed.

This cyclic movement in albuminuria is not peculiar, however, to postural albuminuria. Dr. Pavy, Dr. A. H. Carter, and Dr. Saundby have shown that in chronic Bright's disease there is a similar cyclic manifestation.

Dr. Saundby finds that the after-breakfast urine is most likely to contain albumen, and that the amount of albumen does not depend upon the quality of the food so much as on the period of the day: results agreeing with those obtained by Dr. E. Smith, viz. that the rate of pulse, of respirations, and of excretion of urea were each influenced much more by breakfast than by any other meal; the maxima of all these vital processes occurring together shortly after that meal had been taken, though the food taken at dinner was much more nitrogenous. Dr. Saundby adds that these facts oppose the idea that nitrogenous food increases albuminuria by passing out with the urine as unassimilated albumen; they prove, he says, that the maximum albuminuria occurs in that period of the twenty-four hours in which all of the excretory functional activities are also at their maximum; and the inference Dr. Saundby draws is that the increase of the albuminuria depends upon the increased functional activity of the kidney at that period. The consideration, however, must not be overlooked that the albuminuria and increased functional activity may be co-results of one cause, the albuminuria being perhaps a mere mechanical incident associated with increase of functional activity, and dependent not upon it but upon some primary factor which governs both.

Bartels recorded a case, which is of interest in this connexion. A patient¹ of his was the subject of nephritis, which came on after an extensive phlegmon; and for weeks the urine passed at night was clear and yellow, while that passed by day was blood-stained.

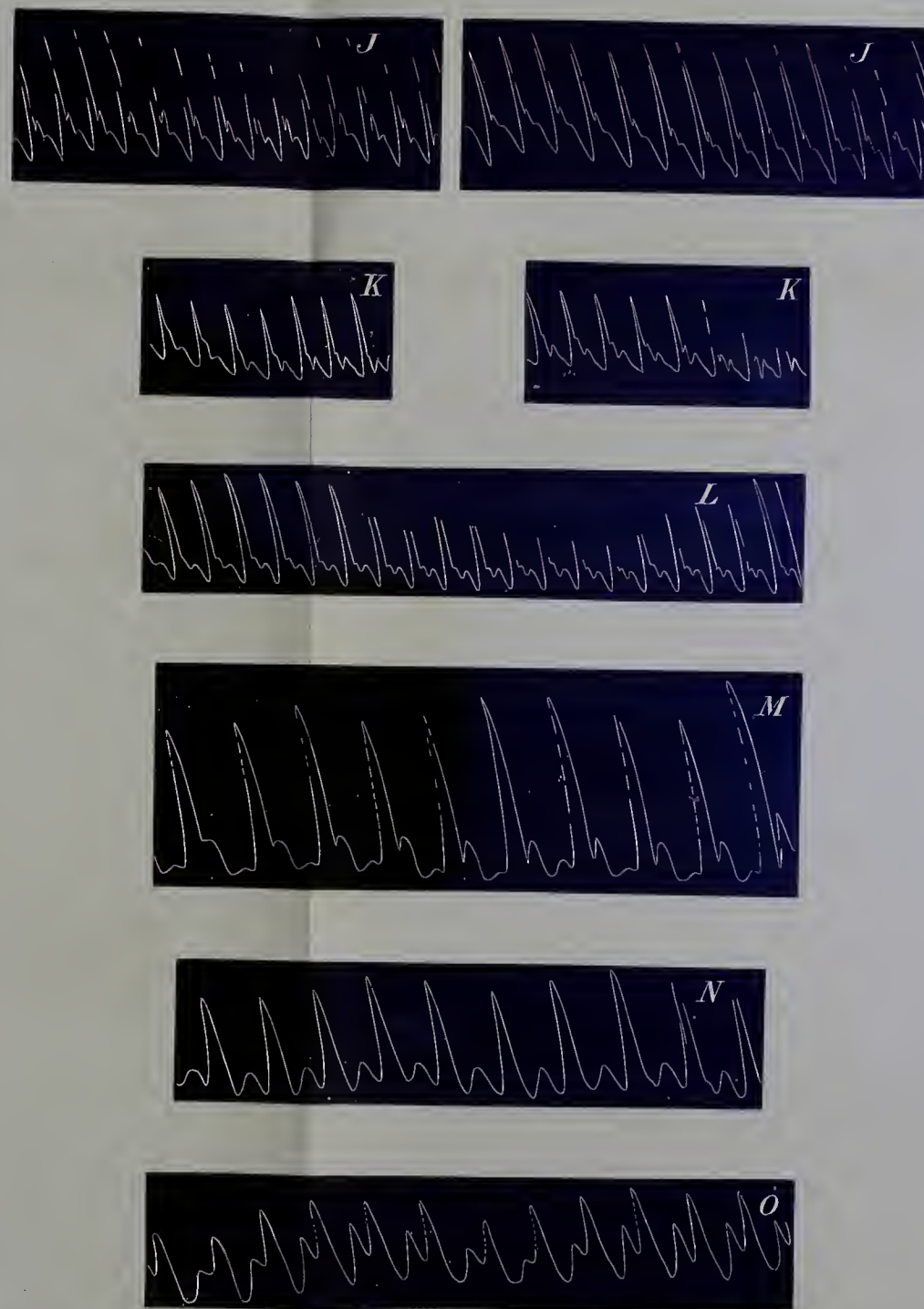
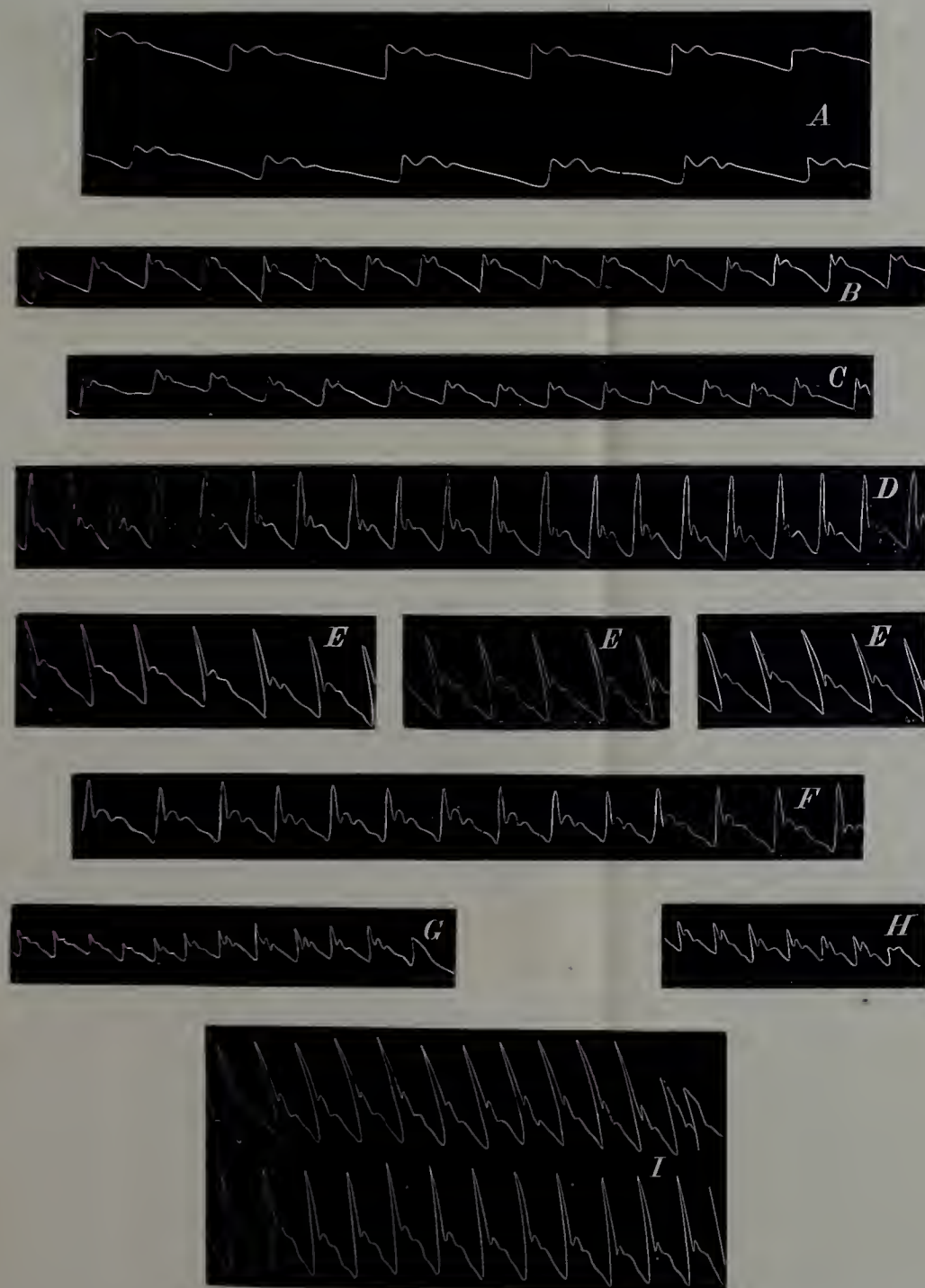
As has already been said, I have regarded many cases of postural albuminuria in the same light as those in which albuminuria comes on only after exercise, ascribing both to an excess of glomerular pumping. In addition to the cases brought in evidence

¹ Einer meiner Kranken, der nach ausgedehnter Phlegmone von Nephritis befallen war, entleerte Wochen lang bei Tage blutigen, zur Nachtzeit klaren, gelben Harn.

already, it may be mentioned that S. Marcacci finds his urine albuminous by day and not by night ; and that the albuminuria may be brought on at any time by rotating his arms for about a quarter of an hour, so that the pulse increases from 75 to 115 per minute. No doubt, as I have said, venous hyperaemia plays an important part in causing albuminuria in some postural cases, as also in other cases in which this symptom does not appear unless such exercise be taken as running a considerable distance, vigorous rowing or climbing, instances of which have been recorded by Dr. B. Dukes, Professor G. Stewart and others. There are, however, other hypotheses concerning exercise-albuminuria: and whereas Dr. Johnson would attribute it in certain cases to irritation of the kidneys by some abnormal product or products of metabolism in muscular and other tissues, Dr. Ralfe would lay stress upon the mere addition of work thrown upon, and irritation therefore of, the secreting cells of the kidney.

[I may briefly here refer to the symptoms and to the prognosis in my cases of postural albuminuria. The albuminuria was in many of them the only abnormality discovered. Red blood corpuscles did not appear in the urine of any of the cases. Some of the patients were animated, observant, and very active; here one and there one slept heavily; they all ate well; there was no complaint of headache or nausea; they all gained weight; their shins were perfectly sharp and their features finely drawn. Tension and persistency of the pulse were in some instances slightly increased. In this connexion attention may be given to the sphygmograms of Plate II—*F, G, H, I, J, K, L*, all of which were taken from persons at the time suffering from postural albuminuria. Those patients, however, in whom the albuminuria did not readily pass off on lying down, generally became anæmic after a week or so; and some of these had slight facial dropsy, and more marked rise of arterial tension, though in no case so marked as at *A, B*, and *C* in the plate.

As to the ultimate prognosis:—this will vary with the view taken as to the cause of the albuminuria. My patients had certainly had organic renal change, such as might in most instances be called perhaps subacute. I have shown that there is strong presumption in favour of Dr. Johnson's view, that in most cases of postural albuminuria there has been, and still is, some



SPHYGMOGRAMS taken with Marey's instrument, except *B, C, D,* and *F,* which were taken with Dudgeon's,—from persons affected with scarlatinal albuminuria *A* to *L* or with other conditions (*M* to *O*). The pressure employed in cases *A* to *L* was 2 ozs., except in cases *C* and *G*, in which the pressure was $2\frac{1}{2}$ ozs.

A to *E*.—Red blood corpuscles in urine; dropsy.

A. Male, 4 yrs.; 2 days after uraemic convulsions. *B*. Female, 7 yrs. *C*. Female, 7 yrs. *D*. Female, 14 yrs. *E*. Female, 19 yrs.

F to *L*.—Postural albuminuria; no red blood corpuscles in urine.

F. Female, 16 yrs. *G*. Female, 8 yrs. *H*. Female, 12 yrs. *I*. Female, 15 yrs. *J*. Female. *K*. Female, 15 yrs. *L*. Male, 19 yrs.

M to *O*.—Introduced merely for the sake of comparison.

M. Aortic regurgitation. *N*. Soft pulse of Peliosis rheumatica. *O*. Hyperdicrotous pulse of enteric fever, 44th day.

change of renal structure. That the kidneys in postural and like cases are not undergoing active changes may be inferred from the want of evidence of such changes in the urine. But are they undergoing slowly progressive changes, which, if not checked, may end in renal disorganization? Dr. Johnson recorded in 1879 the case of a man with advanced granular kidney, and all its accompaniments of vascular and other changes, which he said had originated many years before in scarlet fever, though there had been neither dropsy, nor any other sign of nephritis, in the course of the fever. Dr. Johnson finds that intermittent albuminuria, if neglected, tends to become permanent; and, on the other hand, that acute albuminuria in progress to recovery passes through a stage in which the albuminuria is intermittent. The evidence, in fact, drawn from many sources, tends to show that albuminuria coming on promptly after the assumption of the erect posture, or after exercise, is indicative of actual renal change: and this being so, careful attention must be given to the hygienic, the dietetic, if not the medicinal measures which are likely to reduce the risks of an exaggeration of that change, and to afford time for the renal tissues to revert to their more normal characters.]

CHAPTER V.

ALBUMINURIA OF LATE ONSET.

I HAVE said that in the patients under my care albuminuria of degree above the slightest commenced at various times,—generally, however, during the early part of the first week, at or about the ninth day, the fifteenth, or again the nineteenth, day. Mahomed asserted that it might come on six weeks after attack, or even later; and others have said that the dropsy of scarlatinal nephritis may appear even after the lapse of several months.

In none of my cases did albuminuria, or other evidence of nephritis, come on for the first time after the nineteenth day; and the recorded cases in which evidence of nephritis has appeared for the first time as late as six weeks after the date of attack, are, I think, extremely few.

It is, moreover, of importance to bear in mind that renal lesions may exist, and may be making progress, without albuminuria. Many authors may be quoted to show that albuminuria is not a necessary associate of nephritis. Tüngel in 1861 spoke of an entire absence of albumen from the urine of persons, who after death were found to have true interstitial nephritis. Dieulafoy refers to a person who suffered from mixed nephritis without albuminuria, and to another who suffered from interstitial nephritis without albuminuria except on the day that preceded death: he thinks the absence of albuminuria is more common when the mischief is confined to the renal vessels, than when confined to the tubules. Mahomed, also, showed that albuminuria was very frequently absent in renal cirrhosis. Thomas, again, alluded to the absence of albuminuria in certain cases of scarlatinal dropsy; he averred that even epidemics had been observed in which all the dropsical patients were quite free from albuminuria (p. 243 *l.c.*); but he, nevertheless, thought that scarlatinal dropsy did not occur without nephritis, though he was aware that Frerichs had ascribed certain cases of oedema after Searlatina to an affection of the vasomotor nerves of the skin, as a

result of exposure in the desquamative period and as independent of any renal change¹. Bartels also spoke of the occurrence of scarlatinal nephritis without symptoms ('Nephritis scarlatinosa lange symptomlos verlaufen kann'). Sir Dyce Duckworth has found scarlatinal dropsy associated with but little, if any, albuminuria: and he is in accord with Dr. Gee, Dr. Dickinson and others in referring such cases, when occurring after Scarlatina, to nephritis. Dr. Goodhart relates that a girl, three-and-a-half years of age, who, eight weeks after an attack of Scarlatina, looked 'quite like a distended bladder,' passed urine free from both albumen and casts; this dropsy began about four weeks after the onset of the disease.

May, then, the cases, in which albuminuria is said to have come on after the fifth or sixth week, be explained in this way? Take those recorded by Drs. H. Weber and Oswald Browne.

Dr. Weber reported three to the Medical and Chirurgical Society in 1866. The urine of the patients, severally aged seven, fourteen, and twenty-five years, had been 'examined frequently and carefully from the beginning of the Scarlatina to the end of the fifth or sixth week,' and albumen had not been found: albuminuria, however, appeared afterwards; it was not attended by pyrexia, anasarca, or haemoglobinuria; but it was attended by lassitude, anorexia, swelling of the lymphatic glands, anaemia, eruption of boils, and increased desire to micturate. These cases he would not class with the renal disease of so-called scarlatinal dropsy, owing to the lateness of onset, and to the absence of pyrexia, anasarca, and haemoglobinuria. Dr. Weber had previously had two similar cases, in which, though the albuminuria occurred after erysipelas in the one, and after typhoid fever in the other, the urine had been quite free from albumen during the febrile state and during convalescence. Dr. Browne's case was recorded in 1883. The patient, a girl, seven years of age, was discharged from hospital on the forty-fifth day after the onset of scarlatina; and, though up to that time the urine had been examined carefully every day, 'there had never been even a trace of albumen' detected. On the fifty-fourth day, however, the patient was puffy: on the fifty-seventh, 'much

¹ Acute general dropsy without albuminuria is said by Dr. Johnson to have been observed by Blackall, Roberts, Basham, Dickinson, and Cayley, as also in two cases by himself.

albumen' was detected; and on the fifty-eighth day she was re-admitted to hospital with 'dropsy, blood and granular casts and albumen, $\frac{1}{3}$:' on the sixty-second day, a papular rash, much like measles, appeared on the face and back, without catarrhal symptoms, and without rise of temperature, a rash which faded two or three days later.

It may be asked whether organic changes in the kidney preceded the albuminuria in these patients. This matter is not discussed at length in the papers referred to. If such changes had for some time preceded the albuminuria, it is quite likely that they would have affected in particular the blood-vessels, as has, indeed, been suggested, and the volume of urine passed would have been in all probability reduced; but the volume of urine passed was presumably not diminished. Again, in regard of the presence or absence of albumen in earlier stages, questions may be put as to the reagent used, the time when the urine was passed, the date when the patients were first allowed to get up; for, it appears, from what has already been said, that my patients in the latter part of the fourth week of Scarlatina, or afterwards, might have renal mischief of a sort which, though not revealing itself in albuminuria while the patient remained in bed, promptly did thus reveal itself when the patient got up, the albuminuria disappearing again quickly on resumption of the recumbent posture. In such cases, it is true, albumen had been passed earlier in the illness for at least several days: but in some this albumen was not great in amount; and it was detected, it may be repeated, by means of picric acid. Hence, though the urine of the periods of recumbency at certain stages of cases of scarlatina may not be albuminous, it is necessary to observe that, if the urine passed during the maintenance of the erect posture be not examined, the patient may continue for weeks to get up and be unsuspected of having any renal mischief, may in fact be discharged from medical care, and yet be suffering from renal lesion which, if not of a progressive character, is, no doubt, liable to sudden exaggeration as a result of various kinds of indiscretion.

As regards the view, however, that the renal mischief commenced, in the cases recorded by Drs. Weber and Browne, about the time when the albuminuria was detected, it deserves notice that, with the first appearance of the albuminuria or soon afterwards, there were boils and swellings of the lymphatic glands in the one set of cases,

and a rash in the other case: and it may be that all these affections were due to one cause, say some recrudescence of the virus in the system, or the action of some noxious body absorbed into the circulation from the sites of previous scarlatinal change. Several of my patients, as will be shown in the next chapter, had albuminuria together with enlargement of cervical glands, and other disturbances late in the course of the illness after getting up; and, though in them the albuminuria appeared then either for the second time or in greatly increased amount, there seems to be no reason for supposing that it might not appear thus late for the first time.

CHAPTER VI.

ALBUMINURIA WITH RECURRENCE OF SORE THROAT.

AMONG my patients there were several, in whom tonsillitis came on afresh (after the initial tonsillitis had entirely subsided); and therewith albuminuria either reappeared or became suddenly increased. This recurrence took place in the third week in some, and rather later in others.

Undoubtedly in some instances the exciting factor was some want of prudence, such as exposure to cold; but in the majority of cases I failed to discover any such immediately exciting cause.

Of the recurrent cases there were in all 23; and in only four of them did exposure, sudden change of weather, or other like cause, appear to afford adequate explanation of the recurrence.

What signification is to be attached to the recurrence in the other nineteen patients? May it, for instance, be thought of as due to recrudescence of the scarlatinal virus? The association of albuminuria with the recurrence of sore throat might perhaps be taken as favouring some such supposition, and it may be of interest to see what evidence there is adducible on this subject.

In the first place, it may be remarked that a person, who has passed through one attack of Scarlatina, may, in a comparatively short period, acquire susceptibility to the disease a second, even a third, or it is said a fourth time. This was attested by Rayer, Bateman, Withering, Blackburne, Tweedie, and others. Sir Gilbert Blane recorded the occurrence of scarlet fever three times in a young lady, 'without the least suspicion of ambiguity or possibility of mistake.' The occurrence of re-attack was insisted upon in 1862 by Dr. B. W. Richardson, who mentioned that he had himself three times suffered from the disease. [It may be added that a girl, seventeen years of age, treated at the South Western Fever Asylum for Scarlatina, of which she fell ill in November,

1887, was treated there for Scarlatina in March, 1884.] It is the same with other diseases¹. Re-attack of this sort has no doubt generally been due to fresh infection. There are, however, appearances in some scarlatinal patients which strongly suggest that there is a renewal of the activity of the virus in the body, independent of fresh infection, appearances which go by the name of relapses. Willan recorded that in two cases of Scarlatina anginosa in 1799, 'both of which were violent, and one fatal, the rash reappeared on the fourteenth day of the disease, i. e. seven or eight days after the decline of the primary eruption, and went a second time through the usual course, terminating by a fresh desquamation.' 'In one case,' he said, 'I remember to have observed the same appearance a third time, about the twentieth day of the disorder: whenever it occurs, it makes a lingering, a virulent, distemper.' Thomas spoke of both genuine relapses and pseudo-relapses, the latter of which he called 'reversiones eruptionis.' The 'reversio eruptionis' he described as a second rash breaking out suddenly all over the body after an attack of Scarlatina, having characters very like those of the first rash, but being more roseolous and not so finely punctated, and being followed by a generally copious desquamation. With this rash there was a second injection of the mucous membrane of the throat, and an enlargement of the papillae of the tongue and of the cervical lymphatic glands. He suggested that it was due to a special 'phase of development' of the virus. Murchison also spoke of relapses in two sisters. Trojanowsky was said by Thomas to have recorded a case in which a rash developed on the upper, and afterwards another rash on the lower, half of the body; and Berton a case in which the reverse was seen. I saw two patients, in the course of the epidemic now under consideration, in whom relapses appear to have occurred. One of them, a female aged three years, apparently scarlatinal on admission, developed all the signs and symptoms of scarlatina after she had been in hospital four weeks. The rash on admission was certainly more erythematous than might have been expected

¹ I may quote an instance in which small pox appeared in the person of an inmate of a workhouse. The medical officer performed the operation of vaccination or of revaccination on every inmate save one, an old man deeply pitted with the cicatrices of small pox: this old man was the only other person to manifest the disease, and he died of it. I have been informed of several such instances.

at the stage of her illness ; but on the other hand she, a susceptible person, remained surrounded by scarlatinal patients four weeks before she manifested the symptoms afresh. Again, in the case of a female four years old, and admitted on the second day of illness, the rash disappeared by the fourth day, and the cuticle peeled freely all over the body : then, on the eighteenth day, a red punctate rash appeared again all over the trunk and extremities ; this rash persisted till the twenty-sixth day, and the cuticle peeled a second time, and very freely : cancrum oris was detected on the twenty-ninth day, and the patient died on the forty-third day. It may, however, be questioned whether the second rash, and attendant symptoms formed a relapse in the proper sense of the term ; they may be regarded as due to a septicæmic process.

If these cases of mine, or those referred to by the above authors, be correctly regarded as relapses, we may also expect that minor degrees of relapsing may at times appear. It is likely that albuminuria, associated with recurrent sore throat, should be viewed in this light, as also, late in the course of a scarlatinal attack :—elevation of temperature with recurrence of slight albuminuria ; or enlargement of cervical glands with albuminuria ; or again merely elevation of temperature without other symptoms than those at all times associated therewith, there being no other apparent cause for the rise of temperature. Such relapses and semi-relapses may be due to rejuvenescence of the virus, or possibly to absorption of some local products, resulting from the scarlatinal infection, after the activity of the virus itself has ceased.

I now proceed to consider the nineteen recurrent cases in further detail.

Three of them would, perhaps, be spoken of by some as cases of ‘diphtheria after scarlet fever.’ The first of the three to fall ill a second time of sore throat was a woman aged twenty-six years. When almost convalescent from the second attack, she was removed to another ward. After she had been there a few days, two other females, severally aged eight and twelve years, at the time convalescent and getting up daily, suddenly fell ill with tonsillitis, anorexia, and high temperature : in the course of two days the tonsils, fauces, and soft palate were covered with a thick greenish yellow covering ; and albuminuria, which had wholly disappeared in both, suddenly came on in marked degree, with great profusion

of leucocytes, a few leucocytal casts, and hyaline spherules in the urine. The first of these three patients had some years previously had diphtheria; and it occurred to me that her affection, above mentioned, might be in part diphtheritic, and that she had been the source of the trouble in the other two. The 'membrane' was certainly not very like that generally described as pathognomonic of diphtheria; but I had seen, in the course of enquiries into epidemics of diphtheria conducted in different parts of England on behalf of the Local Government Board, that diphtheritic throat-affection was not always of a particular stereotyped character, the membrane being at times indistinguishable to the naked eye from transparent brown jelly interspersed with pepper grains, or appearing as a mere transparent film like glue. Still, there was no positive indication of diphtheria. The constitutional impression in the two later of the cases was, however, very marked. There were no paralytic sequelae in any of the three patients during their subsequent stay of four and a-half weeks in hospital.

TABLE W.—*Showing incidence of secondary sore throat.*

	Total number affected.	Number in which a determining cause			
		was apparent.	was not apparent.		
			Total.	together with sex-distribution, and percentages which male and female cases bore to the number of males and females at corresponding ages.	
				Males.	Females.
September cases	7	1	6	1 in 85, i.e. 1.1°/o	5 in 76, i.e. 6.5°/o
October „	3	1	2	1 in 74, i.e. 1.3°/o	1 in 68, i.e. 1.4°/o
November „	2	1	1	1 in 30, i.e. 3.3°/o	0 in 26, i.e. 0.0°/o
December „	5	1	4	3 in 37, i.e. 8.1°/o	1 in 21, i.e. 4.7°/o
January „	6	0	6	0 in 2, i.e. 0.0°/o	6 in 27, i.e. 22.2°/o

Hence, of the total number of cases of recurrent sore throat there were four attributable, it seemed, to exposure to cold; and three which were doubtfully diphtheritic. The remaining sixteen were, I suppose, scarlatinal; some possibly rheumatic. The patients were of ages from four to twenty-nine years, most of them from fourteen to eighteen years. The females affected were more than twice as numerous as the males. None of the patients had diarrhoea; only three suffered from marked dysphagia early in the illness; only six had any cervical affection; only one had arthritis: six had been treated with stimulants.

Excluding the four cases possibly traceable to exposure, there remain nineteen. The percentages of the latter to the several monthly sets of cases show (as seen in Chart II and Table E) that there was a steady increase in their relative frequency from October to January. On referring, however, to Table W, it will be seen that the distribution as between the sexes differed considerably from one month to another; the incidence on the females having exceeded that on the males in September, October, and January, and the reverse having occurred in November and December. Venturing on a provisional hypothesis as to the meaning of these facts, I would suggest that the scarlatinal virus attained its maximum of stability in November, and that while in the process of attaining this stability, as also in the process of losing it, the virus manifested itself in its host by periodicity in its processes. Taking the females alone, it appears from the table that the percentage of cases of secondary sore throat was marked in September; it was less in October; there was no case in November, the virus having then attained its maximum of stability and of uniformity of action: then the secondary sore throats increased in December, and became most common in January. On turning to the males, may we suppose that, as regards them, the maximum of stability of the virus was attained earlier, and that the decline of stability of the virus, as revealed by increase of the relative number of cases of secondary sore throat (in fact periodicity in the vital processes of the virus), began in November and further increased in December?

In respect of other symptoms, it has appeared that, when the disease was but little prevalent, the activity of the virus in the system was small: and I am now suggesting that when its activity was small, the virus was in a phase of development in which it manifested a periodicity in its vital processes.

SUBSECTION B.

Albuminuria associated with passage of red blood corpuscles or of haemoglobin into the urine.

I HAVE attempted to show, in what has gone before, that all my patients who presented albumen in the urine were the subjects of nephritis; and it further appears that, when the urine contained red blood corpuscles or haemoglobin, the nephritis was haemorrhagic, for, red corpuscles were incorporated in the matrix of renal casts in every such case.

Red corpuscles in the urine of a person suffering from nephritis no doubt generally proceed, as Dr. Johnson has said, from the glomeruli. They may at times, however, as Cornil and Brault show, come from the intertubular vessels. They may also be wholly of transrenal origin. Bartels, indeed, ascribed all the blood in the urine of his cases of haemorrhagic small pox to haemorrhage from the mucous membrane of the renal pelves: and, as I have suggested already, in one of my patients, who presented profuse haemorrhage from the bowel with a large extravasation of blood in the subcutaneous tissue of the hypogastric and lower umbilical regions, it is quite possible that in some degree the blood corpuscles and haemoglobin in the urine proceeded from a transrenal source. As, moreover, in other systems and organs of the body, when implicated in the course of disease, haemorrhage may occur from here a part and there a part, as is beautifully seen in the lung of pleuro-pneumonia of the ox or of that of pneumo-enteritis of the pig, so also it may certainly take place from isolated parts of the kidney; and we may suppose that blood proceeds not only from inflamed, but also from other, glomeruli by reason of rupture of vessels consequent upon collateral hyperaemia.

For ascertaining the presence of red corpuscles and haemoglobin I used the microscope, the guaiac reaction, and the haemin-crystal test; the latter, however, only occasionally. The guaiac reaction was applied in the wet way. I am aware that this reaction

appears on application of tincture of guaiacum to the cut surface of a rotten apple, or on evaporation of the tincture from almost any surface, even from paper; and it is possible that it may appear in albuminous urine as a result of the presence of fibrin: but, though the blue colouration does not prove the presence of haemoglobin, want of such colouration goes far, if not all the way, towards negativing the presence of this substance, at least in any but the very smallest quantity.

The urine of persons, who suffered from haemorrhagic nephritis, contained red corpuscles, leucocytes, blood-casts, leucocytal casts, and spherules, the relative proportions of these constituents varying widely.

The intensity of the affection, now under consideration, was of many degrees. The albumen was generally abundant at one time or another: the onset of albuminuria might be sudden; its decrease was always very gradual. The red corpuscles appeared for the first time always after the albumen, and the albumen continued to appear in all cases for a longer time than the red corpuscles. Dr. Thomson, however, found in his cases that red corpuscles were present in the sediment both before the supernatant urine had given an albuminous reaction, and after it had ceased to give such reaction.

The red corpuscles appeared in a few instances very quickly after the first appearance of albumen, in others not until a considerable time afterwards. They might be in large or in only small quantity. When present in large quantity, absolutely or relatively, and not as result of respiratory embarrassment, there was (no matter whether they appeared for the first time in the first, second, or third week) marked constitutional disturbance; the urine being largely diminished, pyrexia being high and cardiovascular changes and dropsy being generally very rapidly pronounced. In these cases the red corpuscles appeared very quickly after the albumen, and the leucocytes were relatively few in number. When the red corpuscles were present in relatively small quantity, and the urine was not much diminished, the case was mild, oedema being absent, or very slight and expressed only by obliteration of the facial features, and cardiovascular disturbance being feebly expressed: the blood corpuscles in such cases had appeared at any time in the course of the first four weeks of the illness. There were, also, not a few instances, in which red corpuscles

were present in but the smallest quantity; and in these the constitutional impression was very slight. Between these varieties there were all transitions. There was also another, and distinct class of case. In this class red corpuscles appeared in the urine in considerable quantity; but symptoms, referable to the kidneys, were only mildly expressed: in these cases the presence of red corpuscles in the urine, already albuminous, followed closely upon the supervention of respiratory embarrassment, the result for instance of tracheal obstruction, bronchitis, croupous pneumonia, pneumonic phthisis. I had an example of each of these forms of respiratory trouble, and in all of them the presence of red corpuscles in the urine seemed clearly to be consequent upon that trouble, and apparently due to venous hyperaemia reacting upon already weakened renal glomeruli and tubules. There was, finally, another class in which red corpuscles appeared only occasionally.

The stage of illness, when red corpuscles first appeared in the urine, varied from the third to the thirty-fourth day. In one patient, in whom they appeared on the third day, there was severe general disturbance manifested by high fever, delirium, vomiting, and dropsy; and the urine, which was greatly diminished, closely resembled meat-washings. In another they appeared on the fourth day; they were not very numerous and the constitutional disturbance was but slight, though there was a little facial dropsy. There were very few patients who passed red corpuscles only at intervals.

Statistics relating to the haemorrhagic cases are classed in Table X. Red corpuscles—I had no case in which haemoglobin was found without at some time also red corpuscles—were detected in the urines of 36 of the 378 patients attacked in the period from October to January, i. e. 9.5 per cent. In only two did they appear for the first time after getting up; in one owing, I think, to acute bronchitis, in the other to acute pneumonia of the lower lobe of the left lung. In every case, in which the albumen and red corpuscles were not detected on admission, the appearance of albumen preceded that of the red corpuscles by at least twelve hours, and in the majority of cases by two or even a considerable number of days. As already remarked, however, Dr. Thomson found red corpuscles in the urine both before the albumen reaction was obtainable, and after it had ceased to be obtainable; and, it may

be added, Mahomed obtained his dry guaiac reaction with the urine under like circumstances.

There was facial dropsy in 30 of the 36 patients. One of the patients died; and two made imperfect recoveries. In a boy $1^{10}/_{12}$ years of age the left half of the scrotum sloughed off, and the left testicle was exposed: the scrotum, as commonly happens, healed up under antiseptic dressings, and the boy made a good recovery.

The statements, already made, as to season in relation to scarlatinal manifestations receive further confirmation from the data set out in the table; but in this connexion it is necessary to make allowance for various circumstances. In several of the cases entered in the table it appeared to me that the presence of red corpuscles in the urine resulted from pneumonic phthisis, bronchitis, or other source of respiratory embarrassment, full regard being given to the possibility that the renal affection might have been in these cases the primary mischief. Again, some patients at the time of admission were dropsical and were passing blood-stained urine: and, as is well-known, want of care may make great difference in the matter of renal affection in Scarlatina. There was also one patient, who passed red corpuscles, and these but very few in number, for no more than two days. Hence, in discussing the relation of haemorrhagic nephritis to season, it may be necessary to set these cases aside. There, then, remain for the several successive months the following numbers, viz., 10, 5, 4, 2, of patients, who passed blood-stained urine for a longer period than two days, and were not so affected at the time of admission: numbers which show a slight reduction for December and January as compared with November. These patients were over 1 year and under 20 years of age; three of them were from 1 to 4 years, and eighteen were from 4 to 20 years, so that the proportions borne by these to the total number of patients at these several ages were respectively 4.0 and 6.9 per cent. Twelve of them were males, nine were females. The severity of these cases was also, it seems, related to season. There was but one really very severe case, one which ended fatally. Under the heading 'severe,' in the table, I have included patients who were affected with moderate vomiting, much headache, drowsiness and slow persistent pulse; there were five such. Of 'mild' cases there were eleven; cases, that is, with the albuminuria pronounced, red corpuscles not very numerous,

TABLE X.—Nos. of patients who exhibited albuminuria with passage of red blood corpuscles
or of haemoglobin into the urine.

	Total.	Apparently due to com- plications.	Thus af- fected on admission.	Primary cases developed in hospital.				Primary cases developed in hospital, and thus affected for more than 2 days.		
				Total.	Males.	Females.	Clinically.	Total.	Ages.	
									1 to 3 years.	4 to 19 years.
October cases	15	2	3	10	6	4	Very mild.	4	2	8
November	5	0	0	5	3	2	0	4	0	5
December	11	3	3	4	3	1	1	3	0	4
January	5	1.	2	2	0	2	2	0	1	1
										5 ¹
										3
										2
										0

¹ In two of the other cases the temperature had not been taken with sufficient regularity to determine this.

dropsy not marked or absent, appetite not impaired, and headache but slight or absent. 'Very mild' cases presented no facial dropsy, no drowsiness, no headache, no impairment of appetite, and a normal condition of tongue; they numbered four in all. The progressive fall of severity of these various cases after October is apparent at once on glancing at the columns, which show the time of their attacks, or again at the column which displays the number of patients in whom the temperature rose at the time of appearance of red corpuscles in the urine.

Renal convulsions occurred only among the September and October patients.

The question now arises as to the relationship between cases of albuminuria, in which red corpuscles did, and those in which they did not, appear in the urine, cases in which the appearance of red corpuscles was apparently due to mechanical hyperaemia being set aside. In the former there was generally some facial dropsy; whereas the majority of the latter had no evident facial dropsy. In the former there commonly were, though in varying degree, headache, nausea, anorexia, and brown furring of the tongue; whereas in the latter these symptoms were somewhat rare. In the former there were numerous blood-casts in the urine; in the latter the renal casts were comparatively rare and all of them were leucocytal: the blood-casts consisting probably in chief part of fibrin, the leucocytal consisting of fused spherules. In the former there generally was great, or very great, diminution of urine, and considerable rise of arterial tension: in the latter the volume of urine was generally reduced, but not largely; and though the arterial tension rose in some of them, it never rose high. Whether these differences were due to rapid sealing up of the tubules by fibrin-clots (casts), and more active changes in the walls of the renal arterioles, or to a widespread general affection of the arterioles of the vascular system (heart and vessels) in the former, I shall not stay to enquire. The haemorrhagic cases were not all marked by great rise of arterial tension: the tension varied in the different patients (*vide* plate II, *B, C, D, E*). It was highest where the volume of urine was most diminished, and (as it happened) where red corpuscles and blood-casts were relatively most numerous; it was but very moderately raised so long as the volume of urine was not much diminished, (and red corpuscles

were relatively scanty). Among non-haemorrhagic cases, the arterial tension was slightly raised in some; but in the majority it was scarcely, if at all, raised. The smaller the volume of urine (and the larger the relative quantity of red corpuscles), the higher rose the arterial tension, the firmer and smaller became the radial artery at the wrist, and the greater the tendency to sleep, torpor, and convulsions. Hot baths had a greater influence for good in the haemorrhagic cases; and, whereas meat diet was prejudicial in them, it was of decided value in the non-haemorrhagic cases after the albuminuria had continued for some time.

Are all these forms of albuminuria, whether associated or not with the passage of red corpuscles or haemoglobin into the urine, to be regarded as degrees one of another? Clinically considered, they may readily be arranged into an almost regularly gradational series. It appears, also, that pathologically such a series may be formed: for, though there are two forms of renal affection in Scarlatina (one in which the arterioles, the other in which the glomeruli, are chiefly concerned), and probably a third in which the tubules are chiefly concerned, all sorts of intermediate forms occur.

The Cardio-vascular Phenomena.—The arterial tension, and the impulse of the heart were notably increased only in persons who were affected with haemorrhagic nephritis, and in only six of them. They were moderately increased, also, in eighteen others, affected with haemorrhagic nephritis; and they were slightly increased in six other persons thus affected, as well as in eleven persons who were passing albumen but not red corpuscles. There was no perceptible rise in any other patient. The albuminuric patients, indeed, presented pulses of all degrees as regards tension.

Concerning the relation of arterial tension to albuminuria and renal affections, whether it be, as it were, parental, filial, or fraternal, there has been and still is much dispute. The parental relationship is that which was advanced and advocated by Mahomed; the filial, is that which, proposed by Bright, is perhaps most commonly accepted; the fraternal finds some confirmation in more recent research.

Mahomed's view, that the rise of arterial tension¹ was primary

¹ It may be noted that D. Cotugno, in 1770, referred the presence of albumen in the urine and other fluids of the body, from which he said it was normally absent, to increase of local blood pressure. 'Semper igitur sudat vas, idest, praeter naturae

presents considerations of great interest; and I shall first give attention to what he said on this subject in papers issued at various times, commencing with that published in the Transactions of the Royal Medical and Chirurgical Society in the year 1874.

According to Mahomed, there were in Scarlatina two kinds of renal affection attended by albuminuria. In one the cause was constipation, in the other it was chill. In the former a rise of arterial tension preceded and caused the nephritis; in the latter there was no rise of tension: in the former the mischief began, so to say, in the blood-vessels; in the latter it began in the kidney itself. Some noxious bodies circulating in the system were the primary agents of mischief in both cases, the presence of some of them being due to constipation, that of others to chill: and in the first case a constitutional, in the second a local form, of nephritis arose.

The order of events following upon unrelieved constipation was as follows, viz. (1) rise of arterial tension, (2) transudation of blood crystalloids into the urine, (3) albuminuria with or without continuance of this transudation, and (4) albuminuria with passage of red blood corpuscles into the urine, in fact haemorrhagic nephritis. If, owing to treatment, the fourth stage did not appear, the albuminuria would in time cease; and then there might be a renewal of the transudation of blood crystalloids. Rise of arterial tension was the factor directly concerned in this transudation of the crystalloids and of the albumen. As, also, in order that it should effectually work in this direction, it was necessary that the renal vessels should be simultaneously relaxed [for, experimental elevation of arterial pressure (for instance, by artificial dyspnoea or by galvanization of the spinal cord) caused the kidney as well as other parts of the body to become anaemic, and the flow of urine to diminish; whereas, if, in the course of such experiments, the renal nerves were cut, there quickly followed both engorgement of the kidney with blood and marked flow of urine (Vulpian, Grützner)], hyperaemia¹ of the kidney was postulated. Mahomed would, in fact, call the affection not 'Inflammatory

ordinem, crassiorem lympham et coagulabilem ejicit, cum vi majori consueto distenditur; quam vini infert sanguis appellens, qui solito copiosior sit, aut impetuosior. Itaque vasa plenitudine sudant, sudant cum inflammantur.'

¹ That noxious matters circulating in the blood should have the capacity for causing contraction of the muscle of all vessels except those of the kidney, is quite in accord with what is known of the action of urea, and of the vasomotor mechanism.

Bright's disease,' but 'Congestion under high pressure.' Writing in the 'Lancet' in 1879, and adverting to his paper of 1874, he said: 'I showed' (i. e. in 1874) 'that a great increase of arterial pressure preceded the albuminuria, and was the first symptom of Bright's; and that this was true not only of the acute disease (as in Scarlatina), but also of the chronic and insidious forms of the malady: and I proved that in the early stages of scarlatinal nephritis the arterial pressure could be reduced by the hot pack and purgatives; and I proved the immediate disappearance of albumen from the urine coincidently with this reduction of arterial pressure.' In his paper of 1874 the evidence was, however, drawn especially from scarlatinal patients; his statements concerning other than scarlatinal forms of nephritis being rather those of analogy; and there are several considerations which require to be borne in mind in connexion with this evidence.

1. The existence of blood crystalloids in the urine was inferred from the slight blueing of blotting-paper about a quarter of an hour after the paper had been treated with tincture of guaiacum and ozonic ether, the paper having first been dipped in the urine and dried over a spirit lamp. It is, however, to be noted that guaiacum, left on evaporation from its alcoholic solution, generally of itself turns blue, let the evaporating surface be what it may. That the reaction was not due to free haemoglobin or to red blood corpuscles was indicated by the fact that neither could be detected with the microscope or spectroscope, and by the fact that the reaction faded and failed as soon as albumen appeared in the urine, (as it did very soon afterwards, if the patient were not purged or subjected to a hot bath), again to appear after all trace of albumen in the urine had disappeared. Dr. Thomson did find, as I have said, red corpuscles in the urine before an albuminous reaction could be obtained and also after such reaction had ceased to be obtainable: but the red corpuscles and the possibility of obtaining a guaiac reaction continued presumably throughout; so that Mahomed's cases were altogether different from those alluded to by Dr. Thomson.

2. Evidence that increased tension preceded the exhibition of the above guaiac reaction was not given, no case having been recorded in which the tension, prior to the discovery of the reaction, is spoken of; but, be it noted, Mahomed had observed that the pulse

of Scarlatina was always, except in debilitated persons, one of high tension from the first.

3. Evidence that the transudation of blood crystalloids preceded albuminuria was, as far I have been able to make out, given in only one case, which may be put thus:—

23rd day of illness—constipation, guaiac reaction; 24th, guaiac reaction; 25th, bath given, urine normal; 26th, got up, guaiac reaction, albuminuria and only 15 ozs. of urine passed during the 24 hours; 27th, some increase of arterial tension (shown by sphygmogram), albuminuria, and a bright guaiac reaction; 28th, urine ‘very bloody,’ with many red corpuscles, etc; 29th, sharp purgative administered, after which body-temperature fell to 98·6° F., pulse fell from 116 to 72, blood disappeared from the urine, guaiac reaction however remaining, and albumen in urine being greatly reduced. The condition of the arterial tension, it may be noted, is not mentioned till the 5th day after the appearance of the guaiac reaction.

Another of his cases, bearing on this matter, may be referred to. In this the guaiac reaction appeared with raised tension on the twenty-seventh day; then, after a purge, the reaction disappeared with slight fall of arterial pressure, and the urine became and remained quite normal. These two cases do not of themselves afford much warrant for the hypothesis advanced. As regards, also, the first case, several questions at once are suggested. The small amount of urine on the twenty-sixth day may have been owing to mischief in the kidney, which had been going on for some days; and there is no information given as to amount of urine, or the presence of albuminuria, for the twenty-third, or twenty-fourth, day. Again, no mention is made of any morphological elements in the urine on the twenty-sixth day; and yet their absence would be exceptional; red corpuscles may have been overlooked, and the guaiac reaction may have been due to them. The case, moreover, seems to have been exceptional in other respects; for the urine, which was ‘very bloody’ on the twenty-eighth day, contained no blood (no red corpuscles or haemoglobin) on the twenty-ninth, as a result presumably of the administration of a sharp purge. The persistence of the guaiac reaction, however, on the twenty-ninth day would suggest that the urine still contained either red corpuscles or haemoglobin. In none of my cases, in which red corpuscles appeared in the urine, was purging followed by so marked a change: among my cases, treated in 1881 and 1882, albuminuria with rise of arterial tension came on in several instances, though the bowels had been freely and regularly opened

by means of castor oil; whereas, in other instances, it did not come on though constipation was allowed to continue for days together. Mahomed maintained that 'one day without an action of the bowel caused the symptoms.' His statement, that albuminuria might be made to disappear promptly by a warm bath, I have not been able to confirm; though I have given warm baths to at least 100 patients in the early stages of nephritis. Again, it is difficult to understand why blood crystalloids should transude before the albuminuria appears, and after it has disappeared, and yet not transude during the continuance of the albuminuria, if the transudation be due to raised tension. Does the presence of albumen interfere with the guaiac reaction?

It is, also, difficult to understand why the urine was always¹ diminished when the tension rose, if rise² of tension preceded and caused the renal disturbance, and if the kidneys were in the hyperaemic condition postulated by Mahomed: for under these conditions an increase of the urine would rather be expected.

We now turn to the cases of nephritis, which were described by Mahomed in 1879 and again in 1884 as being unattended by rise of arterial tension. In one there was acute nephritis, with much albumen in the urine, and with some pyrexia; but the pulse at no time was one of high pressure; and concerning this case Mahomed added: 'I think it was a purely renal affection, due to chill and not to blood disorder.' In a second case there was, after Searlatina, severe and prolonged albuminuria, which continued on and off for four years in spite of most careful treatment by diet and other means; at times there were oedema and other grave symptoms: but the pulse was always large, soft, and non-persistent; a sphygmogram taken, when the urine was highly albuminous, was spoken of by Mahomed as that of a perfectly healthy pulse; and the heart gave no evidence of increase of arterial pressure; Mahomed concluded that the renal disease was purely local. In a third case the urine was blood-stained, and there were peritoneal and pleuritic effusions; but a sphygmogram, taken when the

¹ 'The urine is always diminished when the tension of the pulse is increased, whether albumen or crystalloids be present or not, the urea being markedly diminished.' (Mahomed.)

² 'The increase of arterial pressure precedes the appearance of any symptoms of renal disease (e. g. decrease of urine, increase of specific gravity of urine, or dropsy).' (Mahomed.)

patient was recovering, though still suffering from albuminuria and dropsy, was 'one of low arterial pressure, quite unlike that of Bright's disease in its more severe forms.'

After discovering these and like cases, Mahomed concluded that in Scarlatina there were, as said above, two distinct kinds of nephritis, one beginning in the general vascular system, the other in the kidneys: one, so to say, constitutional; and the other, local: one due to constipation, and preceded by rise of aortic tension and the appearance of blood-crystalloids in the urine; the other due to chill, and preceded neither by the transudation of blood-crystalloids nor by increase of arterial tension. He spoke of scarlatinal nephritis with rise, and scarlatinal nephritis without rise, of arterial tension as two wholly distinct sets of cases. I am myself tempted to think that some of the sphygmograms, adduced in illustration of want of rise of tension, do show some rise of tension and some increase of persistency: and Mahomed's cases, like my own, seem to show that scarlatinal nephritis may be accompanied by a very great variety in the degree with which the arterial tension is increased. It seems to me, in fact, impossible to distinguish with precision cases with rise, from cases without rise, of tension: there may be very marked tension, there may be no more than occurs in health, and there may be almost every intermediate grade between these two classes: all my cases, in which the tension was notably increased, were haemorrhagic. In drawing a conclusion as to the value of Mahomed's hypothesis, it is necessary to keep clearly in mind that his contention was, not merely that a rise of arterial tension preceded albuminuria in a certain class of cases, but that it actually caused nephritis. Constipation in Scarlatina may be followed by increase of arterial tension; and no doubt rise of arterial tension, and even dropsy in nephritis may, as observed by Bartels, precede albuminuria; and further rise of tension may increase this albuminuria: but it is another matter to suppose that constipation may, presumably by retention of some noxious body, initiate a condition which ends in nephritis, not by the noxious body acting directly upon the kidneys, but by its primary action upon the vascular system, whereby the arterial tension is directly or indirectly increased. The applicability of the theory to renal cirrhosis is open to still greater question: for, though albuminuria may be brought on or

increased in a person suffering from cirrhosis, (either by reducing or) by improving the action of the heart, there seems to be no warrant for the supposition that the rise of tension is the cause of the renal cirrhosis. Indeed, Mahomed, it would seem, inclined later, at any rate as regards cirrhosis, to adopt the views expressed by Sir William Gull and Dr. Sutton concerning the nature of this affection, laying less stress upon the rise of tension as a cause of the cirrhosis than as an expression of a general affection of the vascular system, renal and other.

This introduces what I have called the fraternal relationship of nephritis and rise of tension. In this connexion it may be repeated that in the cases, in which Klein found marked changes of the middle and internal coats of the small afferent glomerular vessels as early as the second day of Searlatina, he found the same changes in the vessels of the liver and spleen, the changes in the latter causing even obliteration of their lumina. MM. Cornil and Brault, also, it may be remarked, find that in those cases of diffuse nephritis, in which glomerular lesions predominate, there is thickening of the walls of the afferent and efferent glomerular vessels, so marked at times as to obliterate their lumina. Cirrhosis of the kidney, also, has been considered by Sir William Gull and Dr. Sutton as part of a constitutional affection, a fibrosis of the arteries and capillaries of the body generally, including those of the kidney. In persons, too, suffering from amyloid disease, the change, which begins in the vessels, is by no means confined to the vessels of the kidney. All this would perhaps suggest the possibility that rise of tension, when it occurs, is due to changes in the peripheral circulatory tubes, organic of the intima, organic and functional of the media; possibly also to changes of the heart; and that rise of tension and nephritis are thus fraternally related.

It is impossible to accept without reserve the view, taken by some authors, that the cause of rise of arterial tension is wholly functional, and that the effect of a hot bath or of inhalation of amyl nitrite, and the fact that the tension may wholly disappear as the patient recovers, afford sufficient proof of it. The researches of Klein and of MM. Cornil and Brault on the changes in the walls of the arterioles would alone discredit this view. It has been said that the rise of tension occurs too promptly for any arterial change to have taken place; and that cases have occurred in which, though there was rise of tension during life, there was no change of the arteries observable after death; but these statements, perhaps, require confirmation.

It is not necessary that this fraternal relationship should be

regarded as a twin brotherhood ; it may be the relation of elder to younger, or of younger to elder.

There remains, however, the filial relationship, which, proposed by Bright, has been generally accepted : some attributing the tension more directly to circulatory resistance in the kidney ; some to reflex action from the affected kidneys ; some to retention of water, others to retention of noxious bodies, which should be removed, with or without further elaboration, by the kidneys.

Traube laid chief stress on the hydraemia and the obstruction to the renal circulation. Bamberger supposed that, as a result of the hydraemia, there must take place hypertrophy or dilatation of the heart or dropsy. Others, following Bright, have dwelt upon the retention of noxious bodies. These bodies have been supposed to irritate the heart directly (Bright), or to excite contraction of the arterioles, and so to necessitate a more active working of the heart (Bright), or to irritate the renal cells or the vasomotor centre causing arterial contraction and a secondary and antagonistic stimulation of the heart ('the stopcock theory' of Johnson); or to irritate the heart to over-action, and so excite antagonistic contraction of the arterioles ; or to irritate the muscle of the whole vascular system, and so aid the circulation through peripherally obstructed vessels (Dickinson), and in so doing to excite a semi-inflammatory hypertrophy (von Buhl).

The theories, therefore, concerning the filial relationship are mainly either mechanical or irritative ; and to them may be added another, which takes cognizance of both factors, viz. the mechanical storing of fluid in the vessels, and the irritation of the muscle of the vascular system by noxious matters, which, while having this effect, also excite thirst and so add still further to the blood-volume.

Each of these rival theories has been ably advocated. Inability to raise aortic tension by intravenous injection of salt-solution (Cohnheim and Lichtheim) suggests that hydraemia is not concerned in the rise of tension ; whereas the fact that cardiac hypertrophy follows (Grawitz and Israël) either abundant administration of urea or experimental arrest of function of one of the kidneys, suggests that retention of noxious bodies may be concerned therein, though the tension does not rise : but, be it noted, throughout the experiments referred to one kidney at least remained in full activity.

It is true that in acute nephritis the reduction of urine, the rise of tension, and the dropsy¹ generally occur so nearly at the same time, that it is difficult to say what the sequence of events has been; but it is commonly believed that the sequence is in the above order. Many authors have no doubt as to the nature of the relation of the reduction of urine to the dropsy: the causal relation of the former to the latter was, it seems, noted by Hippocrates; as concerns acute nephritis, it was positively affirmed by Bartels². It seems that in acute cases the rise of tension takes place almost *pari passu* with reduction of urine; but the parts played in this rise and in the dropsy by the hydraemia, by the retained solids, and by the degree of permeability of the vessels, are yet in question. In cirrhosis there is rise of tension in spite of a large excess of urine; but this may simply mean that as long as the heart continues to act well, the kidneys suffice to carry off what otherwise would appear as dropsical fluid, an event impossible in acute and chronic diffuse nephritis. In bringing these facts concerning cirrhosis to bear upon those observed in cases of diffuse nephritis, it is further to be borne in mind that the former is a slowly advancing affection; this may be the reason why in secondary atrophy the tension is not so high as it is in cirrhosis, the heart and vessels having been in the acute stages too suddenly and severely tested to allow them to maintain the increase of tension; and it is also to be remembered that, when the heart fails in persons suffering from cirrhosis, dropsy very quickly makes its appearance.

As an objection to the irritative theory, Bartels said that in some cases of cirrhosis there was no retention of noxious bodies. Against this statement there are the facts recorded by Christison, Leichtenstein, and others. Dr. J. T. Maclagan objected to the theory (1875) on the ground that it had not been shown that impure blood circulated with more difficulty than pure blood: but the observations made by Hales ('*Statical Essays*,' 1769), those by Reid and Erichsen on arterial spasm in dyspnoea, those by

¹ The view that dropsy is due to loss of albumen or to weakness is quite inapplicable to the case of a person who becomes dropsical in the course of acute nephritis.

² 'Das Auftreten der Wassersucht ist in allen Fällen von acuter parenchymatöser Nephritis, aus welchen Ursachen sie immer hervorgegangen sein mögen (mit Ausnahme jedoch immer der Cholera-Nephritis), von der Stockung der Harnabsonderung abhängig—das lässt sich in allen Fällen, welche man vom Beginne an beobachten kann, ausnahmslos nachweisen' (p. 254, *l. c.*).

Ustimowitsch and Grützner and by Grawitz on the action of urea, and those by Drs. Lauder Brunton, Cash, and Gaskell, need to be considered in this connexion.

The hypertrophy of the right ventricle would admit of explanation on either the irritative or the mechanical theory. On the former theory, it would be the result of direct irritation of the right ventricle and of association of its action with that of the left ventricle; on the latter it would, like the audibility of the contraction of the left auricle, be the result of backward telling aortic tension.

SUBSECTION C.

Patients presenting neither albuminuria nor other evidence of renal implication. The turning-point of the epidemic.

I HAVE already said that certain of the 588 patients did not exhibit albuminuria while in hospital; their urine, though examined every other day throughout a minimum period of eight weeks after admission, not having exhibited the faintest turbidity on the addition of picric acid. They numbered altogether sixteen, —nine males and seven females. They were attacked between December 2nd and January 9th, and were admitted to hospital as an average on the fourth day of illness.

The maximum temperature during the first week after admission was at or over 103°F . in only four of them; it was below 101.5 in nine. Only two were delirious after admission, delirious for two nights in one case, for one night in the other; only seven were restless after admission, and only one of these for more than twenty-four hours. In only one was the tongue dry. Only two presented cervical adenitis, only one had an ear affection, only one had early diarrhoea. In four there was slowing of the pulse. There was not in any case conjunctivitis, coryza, dropsy, or secondary sore throat. Nine of them peeled moderately; five only slightly, though distinctly; and two not at all: in one of the latter two there was rheumatism of the wrist for a portion of one day, and the temperature reached 103°F .; and in the other there was an affection of the right ear.

The ages of these patients varied between 3 and 36 years. Nine of them were 3 to 6 years of age, and seven were over 10 years of age; the percentage proportion to the total of persons of corresponding ages attacked during the above-mentioned period being in the former case 16.9, and in the latter 15.2.

Only two of them were of one and the same family. Not one

had stimulants before the twenty-first day; and only two of them had stimulants subsequently. Most of them belonged either to the Lambeth parish (nine), or to the Wandsworth and Clapham Union (five).

Hence, age, sex, stage of illness on admission, residence, treatment, none of these had apparent share in bringing about the absence of albuminuria in these patients. Seeing, also, that all persons attacked earlier, in November and October, did have albuminuria, in less or greater degree, season may be suspected of having had its own special influence on the albuminuric manifestations. I have already shown that this was so: and, to the evidence, which has been adduced, I would add the fact that, speaking generally, the patients, who were attacked in October and in the greater part of November, suffered from at least moderately severe albuminuria; whereas the patients, attacked late in November and afterwards, suffered from only mild albuminuria. These facts, concerning severity of the albuminuric manifestations, suggest that the change in liability to albuminuria began before the 2nd of December; and further evidence of this I shall now proceed to bring forward, by referring to the histories of those October and November patients, who appear in Table R (p. 59) to have suffered but slightly from albuminuria.

It will be seen, on turning to this Table, that there were two October and six November patients, who before getting up exhibited albuminuria of a degree less than that represented by the term 'shade³.' The former were Walter S., 5 years, and Eliz. B., 9 years of age. The latter were John M., 2 years, Alb. S., 9 years, Alf. S., 11 years, Chas. R., 12 years, Charlotte D., 16 years, and Alice N., 9 years of age. The former two, however, though comparatively free from albuminuria before getting up, suffered from albuminuria after getting up in such degree that I had them again confined to bed. Of the second group, the first patient was also confined to bed a second time for albuminuria; the urine of the second patient was not examined after getting up; but the other four patients remained free from albuminuria after getting up. In other words, of these eight October and November patients there were four who, having had only slight albuminuria before getting up, remained free from albuminuria after getting up. It is now to be noted that these four patients fell ill at the following

respective dates, viz. Nov. 29th, 29th, 27th, and 30th, a fact which indicates that some extrinsic difference in liability to albuminuria occurred at or about that time.

This indication derives further support from the histories of the seventeen November patients, represented in the Table as not having had albuminuria on first getting up. They were John M., 2 years, Herb. B., 6 years, Walter R., 7 years, Fred. T., 7 years, Alb. S., 10½ years, Robt. K., 14 years, Fred. W., 19 years, Horace H., 35 years, Thos. C., 3½ years, Hy. E., 7 years, Rose E., 11 years, Alf. S., 11 years, Charlotte D., 16 years, Aliee N., 9 years, Fred. T., 36 years, Jane E., 11 years, and Annie R., 9 years of age. The first eight of these, however, though free from albuminuria for a time on getting up, had slight albuminuria on the second or third day afterwards; whereas the remaining nine continued wholly free from albuminuria after getting up. These nine patients were attacked at the following respective dates, viz. November 27th, 28th, 28th, 29th, 27th, 30th, 27th, 26th(?), and the 21st; dates which, save the last, closely coincide with those above mentioned. It will be necessary, however, to learn the meaning of the exception which occurred in the case of Annie R., who fell ill on the 21st. Is her case to be classed as of like kind with that of the others, who were attacked towards the end of November? I think not, and for the following reasons. Between November 17th and 25th (dates equally removed from the time when Annie fell ill) there were in all 18 persons attacked, nine previously and seven subsequently to the day of Annie's attack, and two, Annie included, on that day. Each of these 18, before getting up, had albuminuria (of a degree represented by shade³ of albumen or more); and all, with the exception of one patient, who did not live to get up, and Annie, had albuminuria after getting up. Annie's case thus appears to have been exceptional, as compared with that of patients attacked at about the same time as herself: and the exceptional character of her illness may have been due to the fact that she was treated freely with oxygen inhalations and oxygenated water; seeing that she alone, of all the above-mentioned patients, had the oxygen-treatment. As further tending to show that oxygen had the influence here assigned to it, reference may be made to the nine October patients represented in Table R. as not having had albuminuria on first getting up. These patients were Aliee E. N.,

4 years, Mary O., 6 years, Eliz. B., 9 years, Wm. P., 15 years, Jas. D., 5 years, Wm. T., 10 years, Arch. G., 23 years, Alf. D., 19 years, and Chas. B., 16 years of age. The first three of them suffered from albuminuria after they had been getting up for a few days, but the other six did not; and the only apparent reason for this difference is the fact that the former had not been treated with oxygen, whereas the latter had been liberally thus treated, all of them with oxygenated water, and four of them also with oxygen-inhalations, $\frac{1}{4}$ cubic foot of pure oxygen having been inhaled by each of the four regularly every hour or every half-hour, night and day, for a period varying from six days to several weeks.

Hence, it appears that a difference of behaviour in regard to albuminuria appeared among patients attacked in the last week of November or later, as compared with those attacked earlier in November or in October; and that this difference was apparently independent of all conditions save season. Oxygen had, it appeared, the effect of diminishing the liability to albuminuria; but oxygen was administered almost exclusively to the earlier set of patients, having been ordered for only one (and this one moribund at the time) of the patients who were attacked after November 21st, so that this agent cannot have had anything to do with the diminution of albuminurie incidence among the latter. In other words, the liability to albuminuria was on the wane in the last week of November; those few patients who, attacked previously to that time, were comparatively free from albuminuria, having owed such freedom to the influence of oxygen: and with this there is to be coupled the fact, which has been already adduced, that the other clinical manifestations of the disease changed about the same time.

It may, of course, be suggested that such comparatively sudden change of symptoms was the result of other change of treatment.

As already said, however, medicinal treatment, save in regard of the use of oxygen, was practically the same throughout, from September to February. Ventilation of the wards, also, perhaps draughty, was ample, and as draughty and as ample at one time as another. It is true, a larger number of patients passed through each ward in October than in later months, owing to the fact that a large number (124) were transferred in that month to the Convalescent Hospital at Winchmore Hill, in order to make room for fresh ad-

missions ; there was, too, an occasional slight over-crowding of some wards in that month, an over-crowding, however, which did not exceed that incurred by the introduction of two or three extra infants into a ward of 30,000 cubic feet. The influence of oxygen, above mentioned, shows that these matters have importance. Dr. Sweeting, the Medical Superintendent of the Fulham Hospital, has, indeed, in his annual report¹, recorded his belief that the increased incidence of albuminuria, which he found in 1887 as compared with the incidence in previous years at his hospital, was directly related to a diminution of air space allowed to each patient in that year ; and, he would intimate, to the practice, first begun in September, 1887, of transferring patients, early in their convalescence, to Winchmore Hill, so that each bed was occupied more frequently by acute cases. Like facts have been elicited at the London Fever Hospital. Dr. Thorne Thorne, in a paper on 'Cleanliness in relation to Health,' says that since ample movement of fresh air has been constantly maintained at the London Fever Hospital, the number of patients contracting albuminuria has undergone a marked reduction. He adds, 'there have, however, been occasions when, owing to repairs, periodical cleansings, or otherwise, it has been found necessary, as a temporary measure, to exceed the number of patients properly allotted to one or other ward, and when this has been the case, the practice has almost invariably been followed by an increase in the number of patients exhibiting albumen in the urine.' And Dr. Thorne relates a like experience on the part of Dr. Ashby in the course of an epidemic of scarlet fever in Grantham. It would, however, appear to be impossible that the marked falling off of albuminuria in the December as compared with the November patients at the South-Western Fever Asylum was attributable to any difference in the matter of ventilation or of cubic space, so nearly alike were the November and December patients circumstanced in these particulars : and, though the same may not be true of the earlier months, it is to be noted that over-crowding, to the very limited extent mentioned above, was allowed in only certain of the wards, whereas the fall of incidence of albuminuria affected the inmates of all the wards, irrespectively of age and of sex. Moreover, if such over-crowding and greater rapidity of renewal of acute cases in the wards had to do with the greater incidence of albuminuria, it

¹ Vide Practitioner, XLI. p. 154 (August, 1888).

might be expected that they would also have brought about a greater frequency of urgent symptoms in September and October; whereas the facts recorded in Table E and Chart II show that it was not so. Speaking very generally, and taking one time with another, it may also be noted that straining of hospital accommodation means exceptional prevalence of the disease accommodated, and frequently exceptional severity; and the question arises whether the severity does not belong essentially to the epidemic, and is not largely independent of ordinary differences of conditions under which the patients are placed. It is of interest in this connexion to note the fact, mentioned by the Statistical Committee of the Asylum Board, that the maximum number of scarlatinal patients under treatment in the Board's hospitals on any one day in 1887 was reached on November 23rd.

From the evidence which has now been submitted, I have concluded that as the hospital-scarlatina increased in its prevalence, and as the scarlatinal mortality for the whole of London rose, most of the symptoms increased in relative frequency and severity; and that as the prevalence and the mortality subsided, so also did the relative frequency and severity of most of the symptoms; and, further, that the differences of behaviour as between persons, belonging to one sex and one small age-group, attacked at one time, and those attacked at another time, were due—to some difference in the constitutional proclivities of the individuals attacked, to some difference in the behaviour of the scarlatinal virus, to some difference in the mode in which the individuals were infected, or possibly to more than one of these factors.

SECTION IV.

CORRELATION OF SYMPTOMS.

IN the preceding sections it has been shown that the symptoms, at least the majority of them, manifested themselves with a frequency and a severity, which, after increasing as time went on from September through October and the greater part of November, diminished rapidly afterwards; the frequency and severity of the symptoms going together, *pari passu*, with the increase and decrease of prevalence of hospital-scarlatina.

As seen, however, on referring to Chart II, the symptoms did not in the aggregate take precisely parallel courses; they did not, all of them, rise together and fall together: for instance, cervical adenitis was relatively most common in October, whereas delirium was relatively most common in November; and so with other conditions. It has, also, to be borne in mind that sex and age as well as season had their respective influences on the different manifestations. Take arthritis; it was more common among females: or again, take delirium; it was more common among males than females at ages above ten years. It may be that in this way some compensation took place in the manifestation of symptoms as between the sexes and as between persons of different ages.

Still, speaking broadly, it appears that in the aggregate the various symptoms ran approximately parallel courses in the varying frequency and severity with which they occurred.

In the next section an attempt will be made to learn something more definite as to the cause of these variations: in this section we may enquire further as to the relation borne by one symptom or one set of symptoms to another.

As serving to indicate the nature of this enquiry a few illustrations may be mentioned. That causal relationship may exist between one symptom and another; or rather between the change which gives rise to one symptom, and that which gives rise to another, admits

of no doubt whatever. Cervical adenitis, for example, is in part at least dependent upon tonsillitis; meningitis may be, and almost always is, dependent upon tympanitis. That some scarlatinal manifestations hold fraternal relationship among themselves is, also, no doubt, true; say the tonsillitis and the rash. This is true perhaps of very many of them, even of those which are generally supposed to be the one the cause of the other; say the tonsillitis and the tympanitis, the latter of which, generally attributed to extension of inflammation from the throat, may itself be, I think, very probably in many instances as primary as the tonsillitis or the rash. There are yet other manifestations, which are regarded as holding relation one to the other as cause and effect in the opinion of some, as effect and cause in that of others, or as co-results of a common cause in the opinion of a third set of observers, relations which I have called parental, filial and fraternal: for instance, the relation of arterial tension to nephritis which was discussed in the last section. Again, consider nephritis in its relation to the cutaneous change: some have regarded it as fraternally related to the latter; others have regarded it as the result of inactivity of the skin, and others again as the result of chilling.

Moreover, in attempting to unravel the relation of one symptom or of one set of symptoms to another, it is necessary, as I have said above, to take account of the influence exercised not only by age and sex, but also of that exercised by season. It may be said that, if, as I have represented, the symptoms or the majority of them rose together and fell together, the histories of patients attacked at different times should be at once comparable, one with another. Undoubtedly; but to render the comparison of any worth due regard must be paid to the several conditions which go for something in the production of the resultant. For instance, arthritis occurred at an earlier stage of illness, with less severity, and for a shorter period among the persons who were attacked in the later months: the changes immediately preceding arthritis, therefore, in these patients would not be the same as they were among the persons attacked at an earlier date: some links of the chain of events among the milder cases might, we may suppose, be wanting or so poorly expressed as to be overlooked; or, on the other hand, the true relationship of arthritis to other changes, masked in the more complicated cases, might in the mild cases be revealed. Hence, it would be

necessary to compare patients of one age, of one sex, and attacked at approximately the same time; and to have recourse to comparisons among patients of different ages, of different sex, and attacked at different dates, rather for purposes of verification. Mahomed asked the question why constipation caused nephritis in one, and rheumatism in another, person affected with Scarlatina. Among 652 absolutely consecutive scarlatinal patients arthritis, as I have said, did not occur among those under three years of age; it was much commoner and of somewhat longer duration among females than males; and, at the ages ten to fifteen years, males did not, whereas females did, manifest this affection: and this age- and sex-distribution is largely in accord with observations recorded by Dr. Cheadle on the arthritis of rheumatism when independent of Scarlatina. Hence, when considering the question as to why constipation causes rheumatism in one, and not in another patient, it is necessary, waiving for the present the question as to whether constipation has such influence, to have recourse to the data concerning age and sex. Constipation in my cases might have preceded rheumatism in females of the age from ten to fifteen years; though it did not precede rheumatism in males of that age, rheumatism not having occurred in them. Similarly, it might have preceded nephritis in either sex at ages under three years, though it did not precede arthritis among patients of those ages.

Temperament, constitution and family relationship must, also, enter into the calculation. The great similarity in mode of reaction to the scarlatinal virus on the part of different members of one family is very striking, regard being had to differences of age, sex, and certain other modifying circumstances.

In order, moreover, to obtain a correct insight into the correlation of symptoms, into their coexistences and sequences, we still have much to learn; our knowledge of the mode in which the functions and structures of the several parts of the body mutually assist to preserve the individual in health, being at present but limited.

The correlation of different vasomotor mechanisms is a subject about which we know but little. It is doubtless all but perfect under certain circumstances; but let the circumstances be suddenly and largely changed, and want of due co-ordination is at once manifested: let a person for example from the temperate regions pass quickly into the tropics, and the skin may so profusely perspire that the amount of urine, though drinking water be taken freely, may fall so low and become so dense as to cause actual pain in micturition.

I have elsewhere suggested that there is a correlation between the activity of the general muscular system (the inhibition of the muscle of the arterioles of this system) and the activity of the radiating fibres of the iris. There would be an advantage (as I suggested in 'Some Pathological Bearings of Darwinism') for that individual, in which the field of vision was enlarged by dilatation of the pupil at the moment when muscular action or readiness for muscular action is required owing to the presence, actual or likely, of an enemy. Setting aside local and other evident causes of variations of the size of the pupil, it is curious to note the frequency with which unconsciousness, when not associated with active muscular movements or pronounced failure of the heart, is marked by a contracted state of the pupil, and the frequency with which, should there be active muscular movement or pronounced failure of the heart, there is a dilated state of the pupil: this correlation may be witnessed in the varying phases of the uraemic, the epileptic and the chloroformic states.

In the paper, above referred to, I suggested, also, that increased perspiration, accelerated heart's action, accelerated peristalsis of the intestines, and, I might have added, increased exhalation from the pulmonary mucous membranes, and diminution of secretion of urine were correlated, one with the other: ancestrally perhaps some of these causing the others, now, likely enough, in part or as a whole, set in action by one coordinating mechanism. Symptoms may, also, be inter-related in a very circuitous fashion, the meaning of which may not be apparent. For instance, one of my patients, while in a hot bath and being rubbed over the anterior surface of the right tibia, complained of a peculiar feeling behind the left ear, and I noticed then that this ear was retracted; on rubbing the corresponding part of the other leg the same feeling was experienced behind the opposite ear. Is it possible that such a correlation is or was of advantage? If so, is it an advantage derived from the greater precision with which the movements of an animal attacking the legs or the hind-quarters can be detected when the ear is directed backwards?

Physiology teaches that, when the arterial pressure is raised, the depressor nerve comes into action, and that thus the heart is relieved of the strain otherwise put upon it; but in pathology we learn that the heart under such circumstances is not necessarily thus relieved. Is it likely that under certain circumstances (repletion of the vascular system with water, containing matters which should be eliminated by the kidneys) the latest developments in the complex vascular mechanism of the higher animal are thrown out of action, and that heart and vessels behave as in more lowly developed organisms, both contracting and driving on the blood? Such a view would be in accord with the theory adduced by Dr. Dickinson and by Dr. Goodhart. Not for a moment, however, that it is to be supposed with Dr. Goodhart that there is 'no strife in nature,' and that on this account the 'stopcock theory' is to be rejected; irritation of the renal cells by impure blood or of the muscle of the arterioles of the body may cause such contraction of this muscle that the depressor nerve is unequal to the task of inhibiting that contraction. The 'stopcock theory' as applied in this connexion is, we may say, no more enigmatic than a 'stopcock theory' as applied to the facts observed in the lungs and right heart when the blood is laden with carbonic acid.

Phylogeny, which considered from the structural side has served to explain many abnormal developments that ontogeny alone would fail to throw light upon, when considered from the functional side also, as it needs to be, will no doubt prove of equal value. For a thorough understanding of the processes of disease it is necessary to bring to bear upon the subject as wide a knowledge as

practicable of physiological and pathological processes, both those affecting the race as well as those affecting the individual. Pathology cannot be read by recourse to processes belonging to the individual alone. It is often something superadded, often something subtracted, often something perverted; and, likely enough, there may be reversion of function as there is reversion of structure, when higher and more lately developed coordinating mechanisms are absent, poorly developed, or inhibited in the performance of their functions.

Hence, in order to attempt to make out the way in which symptoms are correlated a wide field of fact is required. A portion of the ground may be broken here, and another portion there; but it is, as yet, at best but a fragmentary process. And, therefore, in adopting the heading given to this section I do so, not with the intention of fulfilling the engagement which it would seem to imply, but rather for the purpose of drawing attention to some of the difficulties which beset the fulfilment of such an engagement. Nevertheless, there are, as I have intimated, some facts here and others there, of which it may not be irrelevant to the general subject-matter of the correlation of symptoms to speak.

Pyrexia, acceleration of the heart's action and of respirations, increase of metamorphosis, delirium, and other frequently concurring manifestations are not in all probability related to one another in precisely the same way in the various communicable febrile diseases: but how they are inter-related, which may be cause and which effect, and which may be in fraternal relationship, are yet open questions. Is the pyrexia the cause of the heart's acceleration, is it the effect, or are the two independent? By some the pyrexia is accounted the cause of delirium: the temperature may, however, be largely reduced by cold water or by salicylate of sodium and yet the delirium will continue; a matter I frequently observed in Scarlatina in 1881 and 1882, as has been observed, too, in other diseases.

It is, also, of interest to note the frequency with which tonsillitis and albuminuria are associated in disease generally; yet it has been said, that when tonsillitis is severe in Scarlatina, there is less likelihood of the occurrence of nephritis. I have not observed such relation in Scarlatina myself. Does it refer to patients of a certain age? Does the term nephritis embrace mild cases as well as severe? If the relation, referred to, be a fact, it is one of interest.

Arthritis was said by Thomas to come on during the decline of the rash, when peeling began. Among my patients, as has already been remarked, the milder the case, the earlier the arthritis mani-

fested itself; and in mild cases peeling commenced later than in those that were severe. Most of my arthritic cases were mild, as has been said above. Neither pericarditis nor any heart affection was observed in any of them; though, as I have said, pericarditis without arthritis occurred in one patient. Dr. Ashby gave an account in the *Lancet*, 1886, of twenty cases of Scarlatinal Synovitis, which, it appears, were all that occurred among 900 patients, whom he had had under observation: there was no evidence of endocarditis in any of them; one had temporary pericarditis and another had pleuro-pericarditis: the synovitis came on generally from the seventh to the ninth day of the illness; and in each case the raised temperature was 'maintained longer than usual on account of severe inflammation of the cervical glands or ulceration of the tonsils or palate.' In my arthritic patients, however, there was no special affection of the cervical glands or of the tonsils; as already said, these patients were lightly affected. Dr. Ashby, it may be added, found that synovitis, indistinguishable from that of true rheumatism, might come on at the end of the third or during the fourth week; and that endocarditis in such cases was not uncommon. None of my patients had synovitis so late in its onset as the third week, except one, a boy, who had suppurative synovitis of the left elbow. It is of importance, in discussing the relation of arthritis to other symptoms, to note the great promptitude with which a relapse of arthritis occurs in persons, who are attacked by Scarlatina when but just convalescent from acute rheumatism. Such ready relapsing occurred in one of my patients: and Dr. Ashby had three patients, in whom either pericarditis or joint-pain occurred within a few hours of the onset of the fever of Scarlatina.

[It has been suggested by some authors, first by Beitz in 1851, that there is essential relation between the cause of acute rheumatism and that of Scarlatina. Dr. Ashby shows that scarlatinal rheumatism is not necessarily confined, as it was in my patients, to persons with initially mild symptoms: and Trousseau showed in 1853 that it might be severe, generalized and attended by delirium; that, indeed, the case might end fatally. Epidemics in America, in the East and West Indies, called Dengue, Scarlatina rheumatica or Exanthesis arthrosia, and characterized by fever, pain and swelling of the joints and scarlet rash, may, it is thought, be related to Scarlatina. Moreover, the age- and sex-distribution of Scarlatinal

arthritis, already given, taken in conjunction with the statistics recorded by Dr. Cheadle concerning non-scarlatinal arthritis, would suggest that in these affections there is a virus with similarity in its capacity for making mischief. Messrs. Buchan and Mitchell and Dr. Longstaff have, also, laid stress upon the similarity of seasonal distribution of deaths from rheumatism and scarlet fever. It would be interesting to know what later sequelae occur in children, who have had scarlatinal rheumatism. Do they tend to develop pericarditis, endocarditis, chorea, rheumatic nodules?]

We may, finally, consider the relation of the renal to the other manifestations. It has been, and still is commonly, said that the former have been absent when the tonsils have been specially affected. C. Wells in 1806 spoke of persons, who were suffering from very mild Scarlatina, becoming dropsical; and of others, suffering from putrid sore throat, not becoming dropsical. J. Blackall said in 1818 that scarlatinal dropsy was especially frequent in persons, in whom the skin rather than the throat had been notably affected; that it occurred in mild cases of Scarlatina, but hardly ever in the severer cases, Angina maligna. It is quite possible that in many of these cases it was Diphtheria and not Scarlatina that was under observation. Some have regarded the nephritis in the same light as the rash, as a desquamative inflammation of the urinary tubules, giving to it the name, Enanthem. Franz Simon in 1844 seems to have taken this view. Dr. G. Johnson in 1846 spoke to this effect; he said: 'As the inflammation of the skin in scarlet fever terminates in an excessive development of epidermis and a desquamation of the surface; so inflammation of the kidneys excites an increased development of the epithelium, which lines the urinary tubes, which accumulates in and chokes up the tubes and is washed into the urine.' Warburton Begbie, again, in 1852 regarded the early albuminuria as due to mere desquamation of the urinary passages, and the late albuminuria as due to renal inflammation excited by chilling during the desquamation of the cuticle. Some have regarded the nephritis as dependent directly upon derangement of the functions of the skin; or upon this and chilling of the surface, to which the condition of the skin is said to render the patient peculiarly liable; or upon chilling alone.

It may, in the first place, be asked whether any help towards solving this problem is to be derived from varying intensities of the two affections, the inflammation of the skin and that of the

kidney. Do the intensities of these affections vary together? It would appear that, generally speaking, they do; they appear to have varied *pari passu* in my cases. In this connexion, Warburton Begbie, writing in 1852, observed: 'I have said that this albuminous condition of the urine' (i.e. the early albuminuria which accompanies desquamation of the cuticle) 'is associated with the cuticular desquamation; it is so in the time of its occurrence, and so it is also as regards its amount, for I have noticed the albumen in the urine to be greatest in amount, and to continue longest, in those cases, in which the process of desquamation had taken place to the greatest extent. In those cases in the urine of which no coagulability has taken place—for my more recent experience has shown me a few such—there has been no very marked desquamation, and no direct evidence of any epithelial separation, as shown by examination of the urine.' He went on to say that in some of the latter cases he had found albuminuria. Thomas found nephritis generally more frequent in severe epidemics; but he thought it independent of the character of the desquamation¹. In my cases the intensity of peeling and of albuminuria very generally went together. In this connexion reference may be made to what has already been said of my patients who did not have albuminuria: for, in their cases, though peeling was of many different degrees, it was not in a single case more than moderately marked. Or, reference may be made to what has been said of albuminuria and peeling among persons attacked in November and December. There were, however, a few, though only a few, exceptions. For instance, a well nourished boy, of very dark complexion, had only the slightest degree of albuminuria and no other evidence of nephritis, but he peeled most profusely; and a brunette presented a similar history. On the other hand a patient, who desquamated very slightly indeed, suffered from haemorrhagic nephritis of some severity. Yet, it was certainly the rule that where the skin affection was marked, the rash long continued, the peeling early and pronounced, nephritis was more frequently present. Moreover, the exceptions here quoted, constitute as great a difficulty in the way of the acceptance of one theory

¹ 'Kein Fall von Scharlach ist vor ihr sicher; sie schliesst sich an normale und leichte, ja die leichtesten und selbst rudimentären, wie an die schwersten Fälle an, ohne Rücksicht auf die Art der Desquamation und die Beschaffenheit und Funktion der Haut' (p. 241, l. c.).

as another. Peeling was, for instance, almost absent in the last of those quoted; and it cannot, therefore, have had to do with the nephritis. Of course, it may be that in this case the patient took on a nephritis, which was not essentially scarlatinal; but, if this view be taken, the case no longer remains as an exception to the first view, the fraternal relationship. Again, if the early albuminuria be ascribed to the pyrexia; and if it is only with the later albuminuria, the later nephritis, that we are to concern ourselves; we may hesitate to accept its filial relationship to peeling; for, there are other factors besides peeling, which may, with equal probability, be causes of such nephritis. If, also, early albuminuria is, as I have supposed it was in my cases, of the same nature as the later albuminuria, then the filial relationship of nephritis, at least when occurring in the early stages, to peeling is again out of court. Or, setting aside mild early albuminuria, there still remain the renal lesions, which may be advanced by the second, third or fourth day, Eisenschitz says even by the twelfth hour, after the appearance of the rash. These can scarcely be regarded as owning filial relationship to peeling, or, indeed, in some cases even to the rash.

The relationship must rather be regarded as fraternal. It appeared to me that, when the rash was specially marked about the lower abdomen and thighs, nephritis was on the whole most apt to occur.

Bartels rejected the view that the skin affection had causal relation to nephritis; for he found that far graver alterations of the skin, following quite as acute and as febrile a course as in scarlatina, were not attended by nephritis. He adopted the fraternal relationship; for, the frequency of nephritis was in his experience related solely to the character of the epidemic. He had had twenty-two cases of nephritis among 180 patients treated by him in the 1853-4 epidemic, and thirteen among 84 patients in 1863; whereas among about 100 patients, at other times under his care, he had met with scarcely a case. He found, also, that the intensity of the nephritis varied with the epidemic; he lost five of the twenty-two patients and all of the thirteen above referred to.

He thought that the nephritis was independent of chilling; for children¹ treated in bed with every care might have it;

¹ 'Ausdrücklich aber muss ich hervorheben dass ich die schwersten Nierenerkrankungen sehr häufig in der Reconvalescenz bei Scharlachkranken habe auftreten sehen,

whereas other children, who passed through their illness so to say in the streets, might escape. He concluded emphatically that it was a specific inflammation¹. He thought the same of diphtheritic nephritis, and the more so after the publication of Oertel's observations in 1871 on the exuberant multiplication of micrococci in the Malpighian bodies and tubules of the kidney of diphtheritic patients: he even supposed that this held of very slight albuminuria, lasting only a short time, in very mild attacks of diphtheria attended by but little elevation of temperature and no disturbance of respiration; though he spoke in no hesitating terms as to the pyrexial origin of early albuminuria in scarlatina. He, also, found the nephritis in diphtheria most marked in persons in whom the symptoms of general poisoning were most pronounced.

It is not, however, to be supposed that the active changes, which end in plugging of the sweat glands (observed by Löschner), that the caking on of the epidermis, profuse peeling, and recklessness in exposure to cold, do not intensify, or even determine the causation of nephritis any the less because the person is suffering from scarlatina. Thomas, indeed, said that after he had commenced to give baths regularly, he satisfied himself that nephritis among his cases was more rare. Some years ago a practice was adopted in a certain part of Lincolnshire of rubbing oil over horses that had just been singed; and several of them died, it was said, of nephritis or of pneumonia, as rabbits have more recently been found to die when varnished. The above conditions serve, however, as little to explain the essential renal lesions of the first stages of scarlatina as they do those of non-eruptive infections, say the diphtheritic or diarrhoeic. Scarlatinal nephritis probably is an infective nephritis due to direct action of the virus or to some direct or indirect products of its activity. Birch-Hirschfeld, Grawitz and Cohnheim find that microbes accumulate in the vessels of the kidney, and that they pass out in the urine, of persons suffering from various infective processes. The observations of Klebs, Letzerich and

obgleich übermässig besorgte Eltern die kleinen Patienten noch gar nicht aus dem Bette gelassen hatten, während verwahten Kinder in der Poliklinik, welche den Scharlachprocess so zu sagen auf der Gasse durchgemacht hatten, verschont blieben.'

¹ 'Dennoch bin ich überzeugt, dass der Process in den Nieren nach Scharlach durch specifische Einflüsse angefaht wird und nicht durch einfach mechanische Störungen des Blutlaufs, wie lang anhaltende Fieberhitze oder collaterale Fluxion in Folge von Erkältung der äusseren Haut sie bewirken könnten' (p. 222, *l. c.*).

Bouchard show that the albuminuria of specific fevers is associated with the presence of bacilli in the kidney. M. Bouchard gave it out in 1880 that he had found microbes in the urine of persons suffering from Measles, Osteomyelitis, Amygdalitis, Erysipelas, Enteric fever, Diphtheritic Angina, Pseudo-rheumatism and other infection-diseases, the microbe having in each case some special form. M. Babes has found microbes in the kidney in cases of nephritis due to yellow fever. Weigert in 1875 found microbes in the kidney in cases of small-pox nephritis. It is of interest, also, to note the remarks made on this subject of infective nephritis by MM. Cornil and Brault. In their work, already referred to, they say: 'Il est plus que probable que toutes les néphrites passagères, observées dans les fièvres infectieuses, sont dues à la présence de bactéries dans le sang, à l'action spéciale de ces bactéries sur le rein et à leur élimination par les urines.'

It is true that some microbes, which are poisonous to the animal, escape by the urine without causing any apparent renal mischief, e. g. the Jequirity bacillus, and, it is said, also the bacillus of acute anthrax in the guinea pig and sheep: but it is possible that something depends upon the actual build of the renal elements, as to whether mischief shall or shall not be worked as the microbe accumulates and finds its way out into the tubules; and it may be upon differences in the structure of the glomeruli as between individuals of different species, or of different ages and sex, that much of the general symptomatology depends.

It may, also, be observed in this connexion that the scarlatinal virus probably passes through certain phases of development in the individual. Certain facts—for instance that correlation of the symptoms varied according as persons were attacked in one month or another; that arthritis occurred more and more early in the history of the case as the epidemic was on the wane; and that nephritis varied in its time-relation to other symptoms in accordance with season, would appear to confirm this view. It may, indeed, be that the virus enters upon a second phase of activity in some individuals, just as the *Bacterium termo* may reappear in the final stages of putrefaction, though it has been lost or almost lost to view in all the intermediate stages. It is possible, also, that the tendency, observed by Dr. S. Ringer, of the temperature to fall on either the fifth, tenth, or fifteenth day of scarlatina is due to

some periodicity in the vital activities of the virus: in similar wise say to the ova-formation which recurs, as shown by the Rev. Dr. Dallinger, at certain intervals, in certain Saprophytes, or to the periodic liberation of ova from the female *Filaria sanguinis hominis*; but further reference to the varying phases of the scarlatinal virus must be reserved for the next section.

SECTION V.

SUGGESTIONS CONCERNING THE RELATION OF SYMPTOMS TO AGE, SEX, AND SEASON.

THERE now remains for consideration the meaning to be attached to the facts, already elicited, as to variation of clinical manifestations in reference to age, sex, prevalence of the disease, and season, regarding each of these as separate and independent factors.

First, however, it may be asked whether the evidence advanced suffices to establish the relations, which have been mentioned, as being real and essential.

Before discussing this question we may perhaps with advantage briefly consider certain circumstances, which either are known, or may be supposed, to have influence on the mode in which an organism reacts to a given infection.

There is the varying constitution of the individual, the primary constitution unaffected by previous infection, and the constitution protected for a certain length of time by previous infection either of the same or possibly of another sort: and there is, no doubt, the varying constitution of the virus, as well as the amount of the dose, and the mode in which the virus enters the system.

The constitution of the individual has no doubt very largely to do with the mode of reaction to a given infection. This is illustrated in the differences exhibited by organisms of different species or orders: such as those shown in the modes of reaction of sheep and oxen to cattle plague and eczema epizoötica; of rabbits and monkeys to the rabic poison; probably of molluscs closely allied, but differing in power to thrive in different media, the one thriving only in fresh, the other only in salt, water, to the larvae of the fluke; differences not more peculiar than those seen when the *Psoroptes ovis* gains access to the skin of a sheep and of a man. It is illustrated in differences incidental to age, such as in the reactions of persons of different age to typhus or

to yellow fever; though it needs to be borne in mind that these differences may in some cases be as accidental as are those between sheep which graze closely and sheep which do not graze closely on pastures infested with intermediate forms of the fluke. It is perhaps illustrated in differences peculiar to sex, as shown by Dr. Cheadle in rheumatism. It is shown, also, as a result of differences of condition of nutrition. Fodor, for instance, finds that certain bacteria, injected into an animal reduced by hunger and cold, are better able to thrive than they are when injected into the animal when 'in good condition'; and it is at least likely that the same is true of many different microbes. It is possible that changes of constitution, dependent upon season and climate, also have their influence; and that in this way the comparative benignity of scarlatina so frequently observed in warm weather as compared with its manifestations in the same country in colder (but not very cold) weather, following a hot and dry season, may be explained, as also the fact that the disease is so generally mild when introduced into certain hot latitudes even among Europeans. It is true, there is but a small basis of fact to advance in this connexion: and it would be of interest to learn more of scarlatina when occurring, as it is said to occur, in South America, Senegambia, and other tropical or semi-tropical countries.

Of special importance among closely related organisms there is the difference attaching to individuals, which have, as compared with those which have not, been subjected to previous like infection or indeed possibly to other disease, infective or otherwise. It is said, moreover, that among closely related organisms there are differences wholly independent of previous infection, of age and sex, and of external conditions. *A priori* this would seem to be quite possible: but several instances, which have been given of such differences, have been found to want authenticity and to teach lessons of some importance.

The African Negro of pure descent was supposed to be insusceptible to the virus of yellow fever, and of malaria. It is said, however, that when these affections are prevailing in a virulent form, the Negro does become infected and manifest such infection. This would suggest that the almost complete immunity in the case of the Negro has been acquired. Moreover, the fact that Negroes of pure descent are more likely to manifest the symptoms of yellow

fever on exposure to the poison after they have passed some years or some generations in more temperate latitudes, in which the disease is not indigenous, suggests that in order to maintain this degree of immunity, it is necessary that the Negro should continue to live in localities in which the virus exists, in other words, that the individual or the race should be repeatedly subjected to the virus. It has been suggested that the dark colour of the skin of the Negro has been acquired as a result of frequently repeated malarial infection in the course of ages. It may, in fact, be questioned how far, in regard to these diseases in man, susceptibility differs independently of protection acquired by previous subjection to the action of the virus or of its products; though natural selection may (as certain facts indicate) have acted more directly.

Indeed, it is quite possible that protection, acquired by previous infection, is much more frequently a cause for benignity or only partial susceptibility in the case of these and other infection-diseases than is generally allowed for. It is so probably with Cholera.

Infection, as is known, of babes and infants in the case of several diseases is but trivially manifested as compared with primary infection in later life. This is eminently so of typhus fever. I believe it is so of diphtheria. I have thought it is so, also, in the case of scarlatina. The liability to scarlatinal infection is said by the Registrar-General to be lowest, and yet the fatality from such infection to be greatest, at ages under a year. One is tempted to ask whether the babe is commonly infected, but manifests symptoms so mild that they are not recognized. Does the virus enter the system, and it or its products pass away, say, through the kidneys, the skin, the bowels, so readily that there remains an insufficient amount of either to effect a perceptible reaction? Does the babe supply pabulum inadequate to the wants of the virus; or do its 'phagocytes' make short work with the invading virus? We can scarcely believe that the babe at the breast of a mother, suffering herself from scarlatina, does not receive the virus of this disease; and yet it frequently enough does not manifest such infection. Similarly with yellow fever; it is comparatively mild in children. And, as regards yellow fever, malaria, cholera or cattle plague, may it be that infants and the young of oxen and sheep living in localities, in which these several diseases are indigenous, become infected in quite early life, at times die (and Negro-children, it seems, may die

of malaria), but generally evince only few, if any, symptoms of disease and nevertheless acquire partial immunity ; so that, should the virus, in the case of each of these several diseases, attain later a more active phase, it will not have so marked an effect upon them as it has in the maiden soil of man or beasts of other races previously uninfected? As concerns scarlet fever, its recurrence, at least in an epidemic form, in certain communities every third or fourth year is suggestive in this connexion, especially having regard to the fact that liability to manifest infection and to die of such infection diminishes after the age of four or five years. Why does this term of years intervene between the epidemic prevalences? The frequency of this interval suggests, also, that diminishing susceptibility and fatality in later life is in some part the result of infection in early life.

Murchison proceeding on the assumption that the case-mortality was six per cent. in scarlatina came to the conclusion that in England many persons must attain middle age without having had the disease. Dr. Whitelegge, also, taking an average case-mortality of ten per cent., and a scarlatinal mortality of one per thousand per annum, concludes that less than two-fifths of the survivors at twenty years of age have been protected. But why take a case-mortality of even six per cent.? It cannot have been so high in London in Sydenham's time. Tweedie quoted Rush to the effect that at the time of a certain epidemic of scarlatina at Philadelphia : 'Such was the prevalence of the contagion, which produced the scarlatina anginosa that many hundred people complained of sore throat without any other symptoms of indisposition. The slightest occasional exciting cause, particularly colds, seldom failed of producing the disease.' 'The same thing,' Tweedie added, 'has been repeatedly observed in this country.' I have myself been assured by medical men that they have witnessed extensive epidemics of scarlatina, as also of other infection-diseases, even enteric fever, in which there has been scarcely a death. Willan spoke of epidemics of scarlatina differing as much in fatality as a fleabite and the plague. Dr. J. Andrew recently related to me an instance, in which about one hundred out of four hundred of the boys in a certain Institution were affected with recognizable diphtheria ; and in which it was found by the Medical Officer to the Institution that, during the continuance of the disease in them

a hundred or so more of the boys had albuminuria (without other symptoms) which wholly disappeared some short time afterwards. He, also, told me of a school where scarlatina broke out: the inmates recognized as having scarlatina were isolated; and, though every care was taken in order to detect and to isolate every person with symptoms definitely suggestive of this disease, peeling not uncommonly took place among those inmates, who, though suspected, had not been isolated. How are the numbers infected in these instances to be reckoned up? Shall the albuminuria and the desquamation be accepted as sufficient to stamp the person as having been infected: and how much further are we justified in going? It is, moreover, well known that persons, who have once had this disease, are not infrequently attacked, as Sir G. Burrows observed, with 'scarlatina sine exanthemate' when exposed to its influence at some future period; and that when thus affected they may and do communicate the disease to others. [If my memory serves me well, I have heard of micrococci being present in the blood of a medical man, who had management of scarlatinal patients, though he evinced no symptoms of infection; the micrococci were not present before he took management of the patients, and they were not found again after having been present for some days.]

Having regard to these considerations, and to the fact that scarlatina is so constantly manifesting itself in England and other parts of Europe; the question may be entertained as to how far benignity of the disease, and insusceptibility of the individual, especially in persons after the age of four or five years, are due to previous infection. It would be of interest to learn more of the fatality at different ages from this disease, when introduced into a country, where the inhabitants had long been free from the disease.

I turn now to consider the varying constitution of the virus, the dose and the mode, in which the virus enters the system.

For variations in symptoms dependent upon mode of infection reference may be made to the tubercle-bacillus, according as it gains entrance *viâ* the digestive or the respiratory tract, or again reference may be made to the poison of enteric fever. As regards dose it may be noted that the bacillus anthracis injected into certain animals in small doses produces no constitutional effect (Fodor, Arch. für Hygiene, Bd. 4, p. 129), the bacillus it is supposed being destroyed by 'phagocytes': again, it has been found

by Arloing, Cornevin and Thomas that the severity of the symptoms produced by inoculating calves and other animals with the bacillus of 'symptomatic anthrax' are in a sense proportionate to the amount inoculated. The degree of concentration of the poison must, also, it seems, be considered, as was shown in reference to typhus fever by Dr. Murchison. Differences in the constitution of the virus have, no doubt, also, marked if not most important influence. The virus of rabies is said to gradually increase in virulence as it is transmitted from rabbit to rabbit, or from guinea pig to guinea pig, and to lose in virulence as it is transmitted from monkey to monkey. The activity of the bacilli anthracis is diminished by subjecting them to the action of corrosive sublimate; so that, when inoculated into the body of a susceptible animal, these bacilli, after being thus treated, work but feebly expressed mischief, and nevertheless render the animal secure, at least for a time, against renewal of the symptoms when reinoculated with bacilli anthracis which have not been thus treated. The poisons of certain communicable diseases no doubt vary in their activities with climatic conditions; and there is reason for ascribing such variation to changes in the virus itself, changes brought about directly or indirectly by the climatic conditions.

The history of yellow fever on board the United States Frigate 'Susquehanna' may, if I read it aright, serve as an illustration. Some thirty years ago the crew of this ship, while in service in hot latitudes, were affected with yellow fever (vide Buck's 'Hygiene,' vol. ii. p. 222). The ship was then kept for two or three winters in the North Seas; and on returning to Greytown the fever broke out afresh, though there was none in that town; she was, then, a second time kept for several winters in the North Seas, and on again returning to the tropics yellow fever broke out once more. Did climatic conditions act directly upon the individuals, directly upon the virus, or indirectly upon the latter by virtue of an action upon some intermediate pabulum? It seems quite likely that the influence of climate was primarily upon the virus, climate acting as it might act upon a seed, an egg or a hibernating organism. Similarly with yellow fever when imported into colder latitudes: it may there manifest itself in man, but it does not maintain itself for any length of time, owing, it must be supposed, to the want primarily of certain climatic conditions, and secondarily of appropriate pabulum, inorganic, or organic, the latter it may be in the form of an intermediary bearer.

Moreover, as already said, the virus of certain diseases is known to acquire increasing virulence when transmitted through certain animals, and to lose such property to some extent when transmitted through others. What, then, more likely than that the virus of scarlatina behaves itself differently in man according as it is

introduced into the system from his fellow man or from some other organism, say the cow: just as Wilson Fox observed to be the case with certain *Tineae*?

It would appear, therefore, that in estimating the influence of age, sex, and season, *quâ* age, sex and season, a great number of contingencies have to be taken into account: and the question may be resumed as to how these several influences acted in the epidemic now under consideration. Was all variety in clinical behaviour dependent upon varying constitution of the individuals, all upon varying constitution of the virus, all upon different modes of infection?

In the first place it is necessary to bear in mind that the patients were hospital patients: and that they were therefore to a large extent selected cases. Mothers are unwilling to part with their babies, and unwilling, even if both they and their babies suffer, to go to hospital. Boys and young men do not so readily lie up as girls or young women; and men and women will not be invalided even at home, much less be sent to hospital, unless there be some urgent cause. The statements made concerning age and sex need, therefore, to be taken in conjunction with the fact that the patients were in the above senses selected. For instance, it has been shown that the males over ten years of age manifested delirium more frequently than the females of the same age, and than males of younger age. These facts may be read as due to selection of cases; and the only inference that they would seem to warrant is that in hospital-scarlatina, of the like intensity to that of the epidemic we are considering, the same experience would probably be repeated. Some of the differences, however, which have been observed as occurring between the sexes, such for instance as the incidence of arthritis, are no doubt peculiar to sex; and the same may be said of some of the differences concerning age.

The relation of scarlatina to age and sex has been variously given by different observers, owing no doubt in no small degree to differences of diagnosis, to want of trustworthy data. Of the more recent writers it may be mentioned that Dr. Richardson in 1862 expressed his opinion that sex had no influence, predisposing or otherwise: 'Male and female,' he said, 'are alike susceptible, if they are alike exposed.' The statistics, upon which this opinion was founded, embraced, however, data which were in all probability¹ not altogether

¹ Some of the data were as follow:—In 1843 in the districts of Kent there were

trustworthy. The Registrar-General has reconsidered the whole of this subject in his Annual Report for 1886 on data taken from more modern observation. His calculations are based on nearly half a million of deaths from scarlatina in England and Wales in the twenty-seven years 1859-85, on the case-mortality among 17,795 scarlatinal patients admitted during the twelve years 1874-85 into the London Fever Hospital and the Metropolitan Asylum hospitals at Stockwell and Homerton, on that among nearly 5,000 cases of scarlatina in Christiania (Norway) during the years 1870-82, and on the returns of all known cases of scarlatina for some large towns in England, in which notification of infectious disease has been for some years compulsory. His conclusions, which show that both age and sex have influence, may be recorded here. They are as follow:—

1. The mortality is at its maximum in the third year of life, and after this diminishes with age, at first slowly, afterwards rapidly.

2. This diminution is due to three contributory causes: *a.* the increased proportion in the population at each successive age-period of persons protected by a previous attack; *b.* the diminution of liability to infection in successive age-periods of those who are as yet unprotected; *c.* the diminishing risk in successive age-periods of an attack, should it occur, proving fatal.

3. The liability of the unprotected to infection is small in the first year of life, increases to a maximum in the fifth year or soon after, and then becomes rapidly smaller and smaller with advance of years. [Dr. Ballard taking 2375 Poor-law cases, that occurred in Islington during the twelve years 1857-68, found that there was ‘a gradual but not absolutely regular progression in the liability to scarlet fever from birth to the fifth year of life,’ the age from four to five years supplying proportionately most of the cases. Dr. Whitelegge taking 6288 cases, notified and representing ‘practically the whole of the cases, occurring in three large English towns, having a population of considerably over half-a-million,’ in the

413 deaths registered to scarlatina, 205 of them among males and 208 among females; and the total population of these districts in 1841 consisted of 232,228 males and 236,885 females. Again, among 34,744 persons who died of scarlatina in several different years (1840 to 1849) in Manchester, Liverpool, Birmingham, London, and for part of the time in the whole of England, males exceeded females at ages under ten years, and females exceeded males at higher ages, to extents which Dr. Richardson attributed to differences of male- and female-population at these respective ages.

years 1885-87, finds that the liability increases to a maximum with age up to the fourth or fifth year; and that, if correction be made for those protected by infection at earlier ages, there is a slight excess of cases in the fifth as compared with the fourth year of life.] Cases in the first year of life are, be it noted, but few.

4. The chance that an attack will terminate fatally is highest in infancy, and diminishes rapidly with years to the end of the twenty-fifth year; after which an attack is again somewhat more dangerous. [Dr. Whitelegge's figures 'seem to indicate a rather higher lethality in the second year than in the first.']

5. The female sex throughout life, the first year possibly excepted, is more liable to scarlatina than is the male sex. [Dr. Whitelegge found 'the same curious exception in the first year.']

6. But the attacks in males, though fewer, are more likely to terminate fatally.

7. Hence the longer an attack is deferred the less likely it is to occur at all, and the less likely it is to end fatally.

It is to be borne in mind that the data, indeed the only possible data, upon which these conclusions are founded, are taken from cases of recognized scarlatina: and that scarlatinal infection may easily pass unrecognized even by those, who are familiar with the modes in which it is revealed. The same is undoubtedly true of diphtheria, varioloid and other infective diseases. It becomes, therefore, all the more important to exercise caution in ascribing any differences observed in hospital to differences owing to age *quâ* age, to sex *quâ* sex.

Let us now turn to the influence of season. My data show much difference in clinical features among members of given age-groups, though of the same sex, according as they fell ill in one month or another, there having been an increase of many of the severer symptoms in relative frequency and severity through September, October, and November, and a decrease in these respects in December and January, the change towards decline having appeared about the end of November. It will be well first to enquire whether these observations were exceptional, whether they accord with those of previous observers. As regards scarlatinal prevalence and mortality, it appears that my observations agree very closely with those which have been put on record; and as

regards severity, also of the disease they agree with the observations which have been made by several writers.

Sydenham spoke of the autumnal prevalence of scarlet fever; he said: '*Searlatina Febris, licet nullo non tempore possit ineidere, ut plurimum tamen exeunte aestivo se prodit, quo quidem integras familias, infantes vero prae caeteris, infestat.*' Dr. Richardson drew special attention to this matter in the '*Aselepiad*' of 1862. He said: 'The concurrent testimony, in fact, of almost all writers on this disease, teaches that it is most common in autumn, next so in the summer, next so in winter, and least so in the spring.' Each of the three epidemics, which he had seen in different parts of England, began in summer, became most violent in the last months of the year, continued into the new year, and died away in the spring. Fothergill, speaking of malignant scarlatina, said: 'Although it survives different seasons, and all varieties of weather to which we are exposed, yet it seems to show itself most frequently in the autumn, and in the beginning of the winter; at least, I have met with more cases from September to December inclusive than in all the other months together.' Cullen names the beginning of winter as the time when scarlatina is most prevalent. Withering speaks of the winter and summer months as favourable seasons; and he records the particulars of an epidemic, which commencing in summer was temporarily checked during the cold winds in October, but recommenced with extreme virulence in the warmer days of November. The epidemic of malignant scarlet fever, that raged in St. Alban's in 1748, is described by Dr. Cotton as occurring in the latter part of the year. The epidemic, recorded by Dr. Peart in 1802, did not become violent and extensive until August (the latter part of it) and September. Willan wrote of scarlet fever: 'This disease, as is usual in the summer months, exhibits a mild train of symptoms, and soon terminates favourably, without producing any material affection of the throat. For some years past, it has always been most virulent and dangerous in the months of October and November, but generally ceased on the first appearance of frost.' Other observers found that in summer the affection of the throat was frequently so trifling as not to demand any particular attention. Haygarth found the disease most virulent in October and November. Again, an epidemic, referred to by Dr. Richardson as having been an epidemic of malignant

scarlet fever, and described by Chomel under the title of gangrenous sore throat, raged in Paris 100 years ago, and was most violent in the months of October and November. Dr. Nieuwenhuys says concerning the scarlet fever in Amsterdam in the year 1834: 'The disease first showed itself, in an epidemic form, in the month of June, 1834, and its victims were—in June 22, in July 32, in August 50, in September 78, in October 136, in November 106, and in December 61.' At Birmingham, Mr. Ryland speaks of cases in 1835 among his out-patients as being—for the first quarter of the year 5, the second 7, the third 35, and the last 59. Dr. Tweedie found the disease most common in autumn, least so in spring. Dr. Tripe found that of every hundred deaths from scarlatina in England and Wales in the years from 1840–56 there had occurred 18.0 in the spring, 23.6 in the summer, 35.2 in the autumn, and 23.2 in the winter. Dr. Richardson shows also by his own calculations, based upon the Registrar-General's reports, that scarlatina causes far more deaths in autumn than at any other time of the year; and that it rages most furiously from the middle or end of September to the middle of November, the largest number of deaths having occurred in October. Taking the years 1840–56 he found that the proportions of deaths from scarlatina for the several successive quarters were to one another as the numbers 10, 9, 11, 14; the greatest mortality being therefore in autumn, and the smallest in spring; the variations in any one quarter from year to year were greatest in winter. Dr. Murehison, again, found that in the twenty-four years ending with and including 1863, the greatest number of deaths attributed to scarlatina in London occurred between mid-September and mid-November, the smallest number in March and the beginning of April; and he obtained the same results for the whole of England. Dr. Ballard in 1869 found the cases in Islington, to which I have already referred, to have occurred as follows, viz. 17.7 per cent. in the first quarter, 17.3 in the second, 29.9 in the third, and 35.1 in the fourth. He added: 'As a rule, then, the disease forms one wave of prevalence in the course of the year, the smallest number of cases occurring in the month of March, the largest in the month of October. Spring and winter are the seasons when it prevails least, and summer, and especially autumn, those in which it prevails most.' Dr. Ballard found that the disease had been *least fatal and serious* on the whole

in the summer months ; and that it had been most fatal in the first quarter of the year ; in spring and autumn the fatality was intermediate, nearer however to that in winter than that in summer. He seems to suggest (p. 191, *l. c.*) that the increase of fatality in winter may be due to greater frequency of renal determination owing to chilling. In my cases it will be remembered that there was a great reduction of albuminuria among those persons attacked in December and January, as compared with those attacked earlier ; and, as may be seen from the Tables, the former months were much colder than the latter ; facts corroborating the view that the renal determination is a specific matter, and reducing the importance of the influence of chilling as a disturbing element in the statistical results obtained by Dr. Ballard. Tweedie, again, observed that the disease was generally of a milder character in the spring and summer than in the autumn and winter. As regards prevalence and mortality, the same tale is told by Messrs. Buchan and Mitchell, and others. The former say in the paper already referred to : ‘Scarlet fever’ (i.e. scarlatinal mortality in London) ‘has its maximum from the beginning of September to the end of the year, and its minimum from February to July. The period of the highest death-rate is from the beginning of October to the end of November, being nearly 60 per cent. above the average, and the lowest in March, April, and May, when it is about 33 per cent. below its average’ (i.e. for the months of the year). ‘In each of the thirty years’ (i.e. from 1845 to 1874) ‘the deaths increased at the time of mean maximum, and in all except four of the years the increase was considerable. During ten of the years, a high death-rate was continued on into the year immediately following, but in every year the deaths became fewer, and steadily, if not rapidly, diminished.’ To this there may be added what these authors have recorded concerning diphtheria ; for, some of the statistics, above referred to, include deaths from diphtheria and cynanche maligna. Concerning the deaths from diphtheria registered during the sixteen years ended 1874 they say : ‘The deaths are above the average, as in scarlet fever, from September to the end of the year. The curve’ (representing these deaths), ‘however, differs from that of scarlet fever, in remaining above the average till the beginning of March. Its maximum and minimum periods are also less pronounced.’ Dr. Matthews Duncan in 1876 published similar curves for deaths from

scarlatina in London for twenty-eight years (1848-75); and the autumnal rise was evident in each of these twenty-eight years without exception. The Registrar-General, also, in his 'Annual Summary for 1880,' published curves of like character for the years 1840-79; and he said: 'Scarlet fever forms a single wave, which beginning to rise about the middle of May, gradually attains its highest point in the forty-third week, or end of October, and then as gradually subsides to the end of March, from which time to the middle of May the mortality line remains pretty much at a level, with insignificant fluctuations.' Dr. G. B. Longstaff, again, showed in 1885 that, as regards scarlatina, the weekly admissions into the Asylum Board's hospitals, and the weekly deaths throughout London, in the years 1875-84, rose at times corresponding closely to those mentioned by Buchan and Mitchell and by the Registrar-General.

With this weight of evidence, it must be admitted that the epidemic, dealt with in this paper, was in no way exceptional as regards the time of its prevalence in a recognized form or as regards the time of its mortality; that it was not exceptional as regards fatality in relation to season; and that it was not exceptional in all probability as regards frequency and severity of grave symptoms in relation to season. It would appear that, in the latter respect and also as concerns fatality, the actual month of the year, in which the disease is most marked, may vary somewhat; that the disease may manifest itself severely for several weeks together; and, if we may place credence in Hebra, we must believe that, on occasion at least, the disease may show but little variation in its intensity from one part of the year to another.

Then, what are we to understand by the term 'influence of season' when applied to scarlatina?

It has been shown that a series of annually recurring events takes place either in man or in the scarlatinal virus or in the mode in which man is infected by that virus; a series which results in an exceptional prevalence of recognized scarlatina and exceptional number of deaths from this disease at a certain period of the year, the increase and the decrease in the prevalence and in the mortality being attended by corresponding changes in the relative frequency and severity of at least most of the symptoms; though the amount of prevalence, and the number of deaths, may be by no means the same year by year, as is illustrated in Charts I. and II.

Is periodicity of this sort dependent upon atmospheric changes, or is it something wholly and absolutely independent of such changes; a periodicity which, recurring at yearly intervals, must perforce be correlated with other annual periodicities, whether inorganic or organic, that obtain?

It is difficult to believe that an organic periodicity—and there is but little doubt that it is organic—of the sort we are now considering is not dependent, directly or indirectly, upon periodicity of meteorological condition.

Other events, which, like this, recur in the individual at certain seasons of the year, or at certain periods of the day, are without doubt related directly or indirectly to events, which cyclically recur in the environment. It seems to be at least a justifiable assumption that variations of scarlatinal activity have dependence upon those of season; and, proceeding on this assumption, we may pass on to inquire into the nature of this dependence. What are the intermediate links of the chain connecting change of environment with change of scarlatinal activity?

May we suppose that atmospheric conditions act directly or indirectly upon the individual, so as to favour the reception and the development of the scarlatinal virus at a particular season? There are, for instance, changes in the circulation attendant upon changes of season; there are changes of food-supply, of occupation. There are changes, too, in other disease-prevalences; and there may be compensation as between the ravages of one disease and another owing to the want of material, the want, that is, of hosts fitting for the cultivation of the virus of one, or more than one, disease. [It needs, however, to be borne in mind that such compensation may be only apparent, and due really to the fact that something has been wanting in the conditions necessary to the perfecting of the virus of one or of another disease: for instance, the fluke-disease and 'verminous bronchitis' do not commonly prevail at one and the same time in the same place, owing, not to want of suitable hosts, but to the fact that the *materies morbi* in these two cases are not present together, the one being absent owing to dryness, the other owing (at least so I have thought) to excessive moisture.] May the above or other such changes in the host have an influence upon its receptivity or capacity for affording a nidus for the development of the scarlatinal virus? It might be asked whether the

autumnal atmospheric conditions worked changes in the individual, which rendered scarlatina more readily recognizable or more fatal, as would seem to obtain with the atmospheric conditions of January in relation to phthisis and bronchitis.

On the other hand, may we suppose that atmospheric conditions act on the virus, directly or indirectly by acting upon its pabulum independent of that supplied by the human organism? For instance, may we ask whether summer heat and light act upon the virus directly, favouring or inhibiting its activity, or indirectly by, say, favouring or inhibiting the receptivity of some intermediate host, in which the virus passes a stage necessary to it, if it is to be able or better able to extend in the human family in autumn? Heat and perhaps light, and moisture, in proper proportions, favour the development of the virus of yellow fever, as they favour the growth of wheat. Each is essential to full elaboration of the wheat grain, and, it may be, to that of this virus. Heat and perhaps light without undue moisture aid the propagation of some ectoparasites, say the warble-fly and the gadfly; but, though heat is of value to the propagation of the sheep-scab, the necessary heat may be more perfectly furnished, or deficiency of it in winter may be compensated for, by exceptional facilities of interchange from sheep to sheep, owing to the folding of the animals on more restricted areas. Heat and light, however, without considerable moisture are not of service in spreading abroad the *Limnaeus truncatulus*, and favouring the prevalence of fluke-disease, though they may be sufficient to aid materially the propagation of the *Strongylus filaria*. There are, also, to be considered the facilities with which infection may be acquired at one time as compared with those at other times; a matter illustrated in the case just mentioned of sheep-scab.

There is, then, a variety of circumstances to be considered in this connexion. As an instance of the difficulty attending the solution of this problem there may be mentioned the difference, *said to obtain*, in the incubative period, and in the symptoms, of *Variola ovina*, according as the sheep is infected in warm or in cold weather: for, whereas in cold weather the incubative period is perhaps twenty days, and the symptoms are mild; in warm weather incubation takes, it is said, perhaps only ten days, and the symptoms are more severe. Is this difference determined by change in the virus or by change in the sheep; or is it due to some differ-

ence in the mode of infection, or, say, in the concentration, or the dose, of the virus taken? If it be in the sheep, does the change of weather influence the condition of the animal directly, say, by rendering the peripheral circulation more sluggish, and so exciting or checking central metabolism, or effecting removal of the virus through the kidneys by increasing the flow of urine; or does it influence the condition of the animal by virtue of necessitating the change of food from the grass of summer to the swedes, the turnips, the cake of winter? If it be in the virus, does the change of weather influence the condition of the virus directly, or indirectly by its action upon some other pabulum in or on which the virus requires to sojourn?

Let us now turn to the facts adducible on this subject in connexion with my cases.

The one fact of paramount importance among those, which have been brought forward, is that the symptoms in my cases became relatively more frequent and more severe and then again less frequent and less severe. Any hypothesis to be accepted must meet this. It seems that we may, bearing this in mind, dismiss, as improbable, the hypothesis that difference of circumstances, and therewith of nutrition, of the persons affected had to do with the time-distribution of the symptoms; and the hypothesis that difference of constitution, owing to partial protection acquired by previous infection or otherwise, had to do with it. If such protection had concern in the matter, it would be necessary to suppose that the less susceptible persons were not only the last, but also the very first, to be affected, the more susceptible being attacked in the mid-period of the disease-prevalence; or, if severity of symptoms indicates want of marked susceptibility, then it must be supposed that the most susceptible were not attacked at the time when recognized scarlatina was most prevalent and scarlatinal mortality was at its height. Different modes of infection may perhaps be similarly set aside.

There remains, then, as far as seasonal variations in the individual are concerned, the question whether atmospheric conditions acted directly upon the individual so as to bring about the results.

If atmospheric changes influenced the course of the illness by operating upon the individual, they must have come into operation between the date of infection and the date of admission to hospital; unless it be supposed that the indoor hospital meteorology

differed to such an extent as to have caused the differences of symptoms which were observed ; so that if we ignore the latter possibility, the question becomes narrowed down to one, asking whether persons, incubating scarlatina in different months, would differ in their reaction, by virtue of the direct influence of atmospheric conditions upon them, so as to explain the seasonal character of the symptoms.

It will be impossible to gauge with accuracy what the resultant action of the various meteorological elements would be. I shall, however, briefly consider the influence of changes of temperature.

On referring to Table Y^I, it will be seen that the air-temperature fell each month progressively from September to January ; and that, whereas the difference between the means for November and October was 4.2° F., that between the means for November and December was much smaller, having been only 2.7° F. This would seem to indicate, if air-temperature had to do with the reaction of the individual to the virus, that there is but a small range of temperature best fitted to the manifestation of symptoms in a person infected by the scarlatinal virus.

On referring now to Table Y^{II}, it will be seen that there are certain difficulties in the way of the acceptance of the hypothesis that external temperature acting upon man determined the result. It will be seen that the temperature did not fall steadily and progressively, and that the fall for the week ending November 19th was much more pronounced than the fall for most of the preceeding weeks. If, then, air-temperature occasioned such difference of body-condition as to determine the mildness or severity of the attack of scarlatina, it might be supposed that evidence of its influence would be found about this time. It will be remembered that the fall in the frequency, and in the severity, of the albuminuric manifestations—and it was the same with other symptoms—commenced in persons, whose symptoms were first apparent on or about November 26th. If, then, it be allowed that the incubation-period was of about a week's duration, the actual *infection* of these persons would not have occurred earlier than November 20th. But, during the incubation of the disease in these persons, and in others, who first showed signs of the disease later, in the period up to December 17th, the air-temperature rose again and considerably. Hence, it would appear that, whereas the symptoms were severe among persons who, having been *infected* in the fortnight (seventeen in the

first, and sixteen in the second, week) that preceded November 20th, incubated the disease while the external temperature for the fortnight was in the first week 45.3° , and in the second 34.1° F., the symptoms were very much less severe among persons, fifty-two, who were *infected* in the three weeks that followed, and who incubated the disease while the external temperature was in the first week 38.7° , in the second 43.3° , and in the third 40.4° F. If it be so, then, with a certain external temperature, 45.3° , the body is

TABLE YII.—*Showing meteorological elements for Greenwich, as recorded under the direction of the Astronomer Royal.*

Week ending	Barometer. Mean of mean daily readings corrected and reduced to 32° F.	Temperature. Mean of mean daily values.		Degree of humidity (saturation equals 100).	Rain col- lected in a gauge whose receiving surface is 5 inches above the ground.	Ozone.
		Of the air.	Of the dew point.			
Sept. 3, 1887	29.4	61.4	55.7	82	1.77	10.3
„ 10 „	29.6	56.7	49.9	78	0.67	5.6
„ 17 „	29.7	53.9	47.7	80	0.55	0.9
„ 24 „	30.1	53.6	46.7	78	0.00	0.4
Oct. 1 „	29.5	49.6	44.8	84	0.32	0.4
„ 8 „	30.0	51.9	45.0	77	0.00	0.3
„ 15 „	29.5	41.1	37.5	87	0.36	0.0
„ 22 „	30.3	43.3	38.8	84	0.00	0.3
„ 29 „	29.9	43.1	36.9	79	0.29	1.9
Nov. 5 „	29.1	45.4	41.2	85	1.82	5.2
„ 12 „	29.6	45.3	42.9	91	0.99	0.4
„ 19 „	29.6	34.1	30.9	88	0.53	0.4
„ 26 „	29.5	38.7	36.2	91	0.31	0.9
Dec. 3 „	29.8	43.3	39.7	87	0.48	1.1
„ 10 „	29.5	40.4	35.9	84	0.47	0.8
„ 17 „	29.4	41.7	38.5	88	0.81	3.0
„ 24 „	29.5	34.9	31.9	89	0.17	0.0
„ 31 „	29.8	32.1	26.3	79	0.02	0.0
Jan. 7, 1888	29.6	40.2	36.8	88	0.40	2.9
„ 14 „	30.4	38.1	37.4	97	0.02	1.0
„ 21 „	30.2	34.7	30.8	86	0.21	1.3
„ 28 „	29.9	40.7	36.0	83	0.10	0.9

peculiarly favourable to the working of the scarlatinal virus; let the temperature fall, to 43.3° , 40.4° , or 38.7° , and the body becomes less favourable, and as the temperature falls still further, to 34.1° , the body again becomes more favourable. It may be so; but it would seem to be more probable—in so far as air-temperature had to do with the results—that it was the virus that was primarily affected. We may, indeed, suppose that the virus attained its greatest

virulence in the first of the above weeks ; that it maintained that virulence in the next week ; and that, owing to the low temperature, to which it was then exposed, it lost thereafter in its virulence though the temperature rose considerably. It may here be noted that Dr. Dallinger found that it needed months to adapt certain Saprophytes (*Tetramitus rostratus*, *Monas Dallingeri*, *Dallingeria Drysdali*) to a heated environment, and that a sudden rise of temperature, was generally highly detrimental to them.

Or, to take a broader view, and one which we are justified in taking, we may ask why the scarlatinal manifestation in man is generally mild in the warm months of summer, severe in the colder months of October and November, then mild again in the still colder months that follow. Is this likely to be owing to change of circumstances in the individuals? It surely seems more probable that the explanation must be sought in some change in the virus ; and the above speculations, regarding changes in the individuals as lying at the root of 'influence of season' on scarlatina, must at least be looked upon as unsatisfactory.

Let us now consider the virus. What evidence is there to show that the 'influence of season' means a change in the virus dependent directly or indirectly on meteorological changes? Facts may first be stated which would indicate the likelihood that the virus itself does undergo change.

The variations, which I have shown to have occurred in the course of the epidemic under consideration, would suggest that such is the case, as also would the variations already mentioned as having been observed by previous writers. There are, also, variations of fatality and of certain symptoms, such as albuminuria and dropsy, as recorded for different epidemics, confirmatory of this view. Sydenham spoke of scarlet fever as a disease which 'vix nomen morbi merebatur'; and Morton, referring to it in the same localities two years later, described it as very severe. Graves, also, spoke of the remarkable diversity in character of different epidemics. He said: 'In the year 1801 scarlet fever committed great ravages in Dublin, and continued its destructive progress during the spring of 1802. It ceased in the summer, but returned at intervals during the years 1803-4, when the disease changed its character; and although scarlatinal epidemics recurred very frequently during the next twenty-seven years, yet it was always in the simple or mild form,

so that I have known an instance where not a single death occurred among eighty boys attacked in a public institution. . . . The long continuance of the period during which the character of scarlet fever was either so mild as to require little care, or so purely inflammatory as to yield readily to the judicious employment of an antiphlogistic treatment, led many to believe that the fatality of the former epidemic was chiefly, if not altogether, owing to the erroneous method of cure then resorted to by the physicians of Dublin, who counted among their number not a few disciples of the Brunonian school. . . . Experience derived from the present epidemic (1834-35) has completely refuted this reasoning, and has proved that, in spite of our boasted improvements, we have not been more successful in 1834-35 than were our predecessors in 1801-2.' Copland, in 1858, said, 'There is no kind of fever which displays a greater diversity in its nature and complications, according to the prevailing epidemic constitution, than scarlet fever, or which manifests the character of such constitution more remarkably than it.' In fact, this diversity is attested by almost all who have written on scarlet fever. Hirsch speaks of the fatality of the disease varying from almost nil in some epidemics to three, five, or even thirty per cent. or upwards, in others.

Records of variations of particular symptoms and determinations have in the main been concerned with albuminuria and dropsy. Copland said that, whereas M. Solon found albuminuria in twenty-two out of twenty-three cases of scarlatina, Philipp observed in Berlin at least sixty cases without albuminuria. Thomas spoke of the incidence of albuminuria¹ and dropsy as varying greatly in different epidemics; he quoted Steiner to the effect that nephritis varied in different epidemics from five to seventy per cent., and said that other authors referred to still wider extremes. There is general agreement that this renal mischief is part and parcel of the disease, its frequency and severity varying with the general severity of the epidemic. Thomas² took this view. Bartels, also, regarded nephritis

¹ 'Die Häufigkeit des Hydrops und der Albuminurie in den Scharlachepidemien ist sehr verschieden: während die Einen berichten, dass fast alle Kranken hydropisch wurden, sagen die Anderen dies nur von einer bedeutenden Minderzahl. Nach Steiner schwankt die Häufigkeit der Nephritis parenchymatosa in den einzelnen Epidemien zwischen fünf und siebenzig Procent; nach Anderen kann er noch seltener, aber auch noch häufiger erscheinen.'

² 'Im Allgemeinen aber kommt Nephritis häufiger in solchen Epidemien vor,

of scarlatina, and of several infectious diseases, in this light. He mentioned that he had had twenty-two cases of nephritis among 180 scarlatinal patients in 1853-4, also thirteen among eighty-four patients in 1863; whereas among about 100 patients at other times he did not remember one case (p. 221, l.c.); and that, as regards severity of these cases, he had lost five of the twenty-two and all of the thirteen. Again, Dr. Sweeting, Medical Superintendent of the Fulham Hospital of the Asylum Board, has reported, as I have already said, a greater relative incidence of albuminuria among the patients admitted in 1887, as compared with that among patients admitted in previous years. As, too, the incidence of albuminuria has been found to vary in different epidemics, so also it may vary in the course of one epidemic; among my cases, for instance, there having been a great reduction in December and January as compared with the two preceding months. Dr. O. Fowler, in 1879, said that at the London Fever Hospital there was less albuminuria in winter than in summer.

For variety in the scarlatinal manifestations such as that above shown it seems, then, to be fair matter for speculation as to whether the cause be not variation of the virus itself. That the virus does undergo changes in its development has been supposed by several observers. Thomas¹ referred the 'reversio eruptionis' to a peculiar phase of development of the contagium; as also he did the appearance² of a miliary exanthem in scarlatina. More generally in regard of the contagia of communicable disorders, Sir William Roberts, in his Address to the British Medical Association in August 1877, spoke similarly. After speaking of the laws of heredity he said: 'Cholera suddenly breaks out in some remote district in India, and spreads from that centre over half the globe.

welche man wegen der durchweg schweren Fiebererscheinungen und der sonstigen gefährvollen Complicationen (insonderheit Diphtheritis) womit die einzelnen Fälle verlaufen, als bösartige bezeichnen.'

¹ 'Vielleicht beruht es auf einer besonderen Entwicklungsphase des Scharlach-contagiums,' p. 256, l. c.

² 'In manchen Epidemien war die Entstehung von Frieselbläschen auf der Haut bei den Kranken so allgemein und die Frieseleruption so reichlich, dass das normale, nicht frieselartige Exanthem geradezu nur in der Minderzahl der Fälle beobachtet wurde und die Frieselbildung als Characteristicum der Epidemic gelten konnte: man hat früher solche Epidemien als Scharlachfriesel—oder schlechthin Friesel-epidemien bezeichnet, und ihnen schliesslich auch wohl irrthümlicherweise eine besondere Form des Contagiums, eine Abart des Scharlachcontagiums, zugeschrieben.'

In three or four seasons the epidemic dies away, and ceases altogether from among men. A few years later it reappears and spreads again and disappears as before.' Does not this look, he said, as 'if the cholera virus were an occasional sport from some Indian saprophyte, which by variation has acquired a parasitic habit, and having run through countless generations, either dies or reverts to its original type?' Thorne Thorne¹, indeed, in 1878, was led by his actual experience of diphtheria to the conception of a 'progressive development of infectiveness' in the virus of this disease. Airy¹, in the same year, advanced similar views with regard to contagia in general. Power's pregnant paper¹ in 1879, on 'Thoughts on the nature of certain observed relations between diphtheria and milk,' has direct bearing upon this subject. Hans Buchner, in 1880, averred that he had even transmuted the *Bacillus anthracis* into the *Bacillus subtilis*; and that he could cultivate in the latter the virulent properties of the former. Dr. Klein has shown good reason for doubting the validity of this statement; but we must not forget that analogous changes seem to have actually been effected, such as the increase and decrease of virulent properties of the rabic poison when transmitted through one series of animals or through another.

That the virus of scarlatina is a living entity, as Plenciz supposed it to be (*semina animata*), and capable of multiplying its kind, there can be but little doubt, not merely on grounds of analogy, but also on grounds of observation. It is true that many diverse microbes, which have been found by different observers in the epidermis, the tonsils, the liquid of purulent arthritis, in the inflamed tissue of the neck, and in the urine of scarlatinal patients, have not been shown to have specific pathogenic characters: but Klein has obtained a microbe from vesicles on the teats, and from the tissues, of certain cows, to which Power had clearly traced the infection in a large scarlatinal outbreak in 1885; and he finds that it has the same characters, morphological and cultural, as those possessed by a microbe, which he had previously, and has since, obtained from the blood and tissues of scarlatinal persons, being 'identical in all respects with' it; and that, whether injected into, or ingested by, calves, guinea-pigs, or mice, it brings about symptoms essentially those of scarlet fever in man.

¹ Vide 'Transactions of Epidemiological Society.'

[While these pages are passing through the press, Dr. Klein has shown that, by inoculating his scarlatina-streptococcus into recently calved milch cows, visceral lesions with teat-eruptions are produced, identical with those above referred to.]

Concerning, then, variability in the scarlatinal contagium, there are these several matters to be borne in mind. Moreover, there are analogous facts known of other pathogenic and non-pathogenic organisms. There are, also, the periodically recurring manifestations of syphilis, of ague, the relapses of relapsing fever, of enteric fever, also I have supposed ('Transactions of Epidemiological Society,' 1886) of diphtheria, and, as I have suggested above, of scarlatina. MM. Cornil and Brault, in alluding to the idea that the forms of micro-organisms are constant and immutable, say: 'Tout au contraire, on sait que certains micro-organismes revêtent des formes diverses pendant leur développement, de telle sorte qu'ils se présentent comme un coccus, un bâtonnet, un filament ou une spirale dans les états successifs de leur accroissement . . . En un mot la forme des bactéries n'est pas toujours la même et une espèce donnée présente une certaine polymorphie, dans des limites restreintes, il est vrai.' M. Babes, also, speaks of changes of form of pathogenic bacteria when cultivated in different media.

Seeing, moreover, that (as indicated by Dr. J. K. Mitchell in 1849) it is probably not¹ so much the infecting agent itself that

¹ The antidote may be something which inhibits the activity of the virus, such as, in some instances, corrosive sublimate, or the very products of the virus such as M. Pasteur believes he uses in hydrophobia, or again perhaps some body allied to these products; it may be something which effects the removal of the virus or its products, or again, something to neutralize those products as lime serves to neutralize the oxalic acid produced in the tissues of certain plants. It has long been known that the activity of various ferments, both inorganic and organic, is inhibited by the presence of an excess of the products which are formed in the course of their activity: Pasteur interprets his results in the treatment of hydrophobia in the same way; the same interpretation would apply equally to the treatment for hydrophobia mentioned by Pliny in the words ('Nat. Hist.' Lib. xxix. Cap. v.): 'Saliuntur et carnes eorum, qui rabidi fuerunt, ad eadem remedia in cibo dandae;' as also to the treatment, practised even to the present time in some parts of England, of ague by administration of the urine of the person suffering from ague. Quinine acts, we may imagine, in essentially the same manner as that ingredient in the urine of the aguish patient, which has the same or the like effect, and which has doubtless been produced by the activity of the malarial poison in the body; it may be noted, too, that it is an alkaloidal substance produced by a member of the vegetable kingdom, just as the remedial agent in the urine is likely enough an alkaloidal body produced by the activity of the malarial poison. It may, also, be that in like fashion the units, which go to form a living organism, in the course of certain irregular phases in their history, evolve products, which have a given abnormal action, say that of causing

causes the mischief as the products of its activity ; it may be noted that the nicotine of the tobacco plant of our latitudes is probably very different in amount, if not in actual arrangement of its radicals, from that of the plant grown elsewhere ; and it may be questioned whether this is not in chief part a result of change of meteorological condition. The *Amanita muscaria*, used in Siberia as an intoxicant, grows as an active poison in the south of Europe. So, also, the horseradish dug up in winter has not the pungency that it has in summer. There are numberless instances of this kind both in the vegetable and animal kingdoms ; the products of the metabolism varying markedly in accordance with changes of environment. No doubt, the same holds also of pathogenic organisms, as a result, direct or indirect, of the action of meteorological conditions.

It will now be well to enquire more particularly into the meteorological conditions which constitute the seasons, and see whether any more definite relation than that already indicated has concern with scarlatina, in its prevalences, its severity, and fatality. This subject was examined by Dr. Richardson in the year 1862 ; but his conclusions were negative. He said : 'I sought through the meteorological changes of season for an explanation of the prevalence of this one disease on particular grounds. . . . It was a painful failure, this research.' Dr. Thomas¹ spoke in much the same terms concerning the influence of meteorological condition. Dr. Richardson, however, had examined only the meteorological elements of the time when the deaths were registered ; and the statement of Dr. Thomas concerning them would also seem to apply only to the periods of scarlatinal prevalence. Dr. Richardson found that for each of the twelve weeks severally ended on January 17 and April 4, 1846, January 2, April 10, June 19, July 3 and 31, and August 7 and 14, 1847, and January 11, February 1, and March 15, 1851, there were sixteen deaths in London registered as due to scarlatina ; and yet that the several meteorological elements for these weeks varied widely. For instance, the mean weekly temperature varied as much as 36° F. ; the barometric readings varied between 29.4 and

convulsions ; and that these products themselves when present in excess, or indeed other convulsants introduced into the body, inhibit further action of those units in the same direction.

¹ 'Höchst wahrscheinlich ist dessen Verbreitung vollständig unabhängig von Temperatur, Feuchtigkeit der Atmosphäre, Luftdruck, Windrichtung und Elektrizität der Luft.'

30.2; the rainfall varied from nil to 1.61 inches; the atmospheric movement varied from a calm to 207,857 miles per day; and, though the electric condition of the atmosphere was positive for most of the time (viz. for fifty-seven days out of the twelve weeks), yet in one of the weeks there was positive electricity for only two days and no electricity for the other five.

These meteorological data, however, could scarcely be thought of as themselves influencing, or as affording any indication of prior meteorological conditions that influenced, the virus: they might perhaps be thought of as possibly having influenced some of the individuals affected; and might, therefore, have been considered when the influence of atmospheric changes on the individual was being discussed; but, even in the latter connexion, if they teach anything, they teach only that persons, when infected with scarlatina, die despite wide variations of atmospheric condition. Seeing, however, that as Dr. Richardson pointed out, the meteorological data actually belong to a time probably in many instances belonging to the period after that, in which the patients died, it may be questioned whether they have any direct bearing on the question before us.

Dr. Ballard (l.c.) found that the extension of scarlet fever was favoured 'by a temperature above the average for the season, and that a dry state of the atmosphere, with little rain,' favoured 'its spread more than the reverse condition.' In the autumn, however, he found the disease 'most prevalent in the years when the seasons on the whole were warm and rainy.' Dr. G. B. Longstaff, again, in the 'Transactions of the Epidemiological Society,' 1880, p. 429, drew attention to a connexion of great interest between the mortality from scarlatina, as also from certain other diseases, in England and Wales, and the rainfall at Greenwich. He said: 'As the curves' (i. e. of mortality from these diseases) 'as a rule rise in autumn and remain high throughout the winter, one is naturally led to suspect that a very hot summer or a very hard winter is the determining cause. So far as I have been able to investigate the point, it does not appear that the heat of summer or the cold of winter varies either in a direct or inverse relation to the fatality of these diseases. But when the amount of rain is examined from this point of view, it appears that there is some kind of inverse relationship between the two. The same result is obtained whether the amount of the

fall or the number of days on which it falls is considered, the connection being somewhat closer in the latter case... This relationship is nearly as close as that which subsists between diarrhœa and hot summers on the one hand, or bronchitis and cold winters on the other; and it is strikingly suggestive. How the rainfall influences the fatality of these diseases (we do not know how far it may be related to their prevalence) is another question. Assuming the connection to be real, it seems most natural to suppose that the frequent fall of rain purifies the air, and removes from it the particles of contagious matter; that it acts, to some extent, as the process of inunction in scarlatina is supposed to act, by making the poison less diffusible. Anyhow, the seasonal prevalence of these diseases is not at all obviously related to the rainfall.'

I was not aware, before I had drawn up the statistics presented in Chart I. and Table Z, that this inverse relation between rainfall and scarlatinal mortality had been observed; otherwise I should not have drawn them up in the form in which they now appear. I was led to consider the rainfall owing to the following circumstances. It will be seen from Chart II that of the London scarlatinal patients in the year 1887 the average daily number admitted into the Asylum Board's hospitals, and the average weekly number of deaths, began to rise when the mean monthly temperature rose above 42° F.; that all three (admissions, deaths, and temperature) rose together till July, when the temperature reached its maximum for the year, the number of admissions and of deaths continuing, however, to rise progressively till October, the deaths moreover increasing further in November; and that the admissions and deaths rapidly decreased afterwards. Seeing, too, that the average number of hours per week of bright sunshine also rose till July, after which it rapidly fell off, it occurred to me that heat, bright sunshine and want of rain might have been concerned as factors determining or favouring the perfection of the scarlatinal virus.

On referring to Chart I, it will be seen that the data there given bring into remarkable prominence the constancy with which scarlatinal mortality has risen in dry, and fallen in wet, years: when the rainfall has been very heavy, scarlatinal mortality has been low; and when the former has been very light, the latter has been high.

This association is perhaps more clearly shown in Table Z., in which the average yearly rainfall for succeeding groups of years at

Greenwich, and the average yearly mortality from scarlatina in London for the same groups of years, are set out. The average yearly mortality for the three years 1862-64 was high, the rainfall low: during the next three years the mortality was low, and the rainfall high: and similar alternation may be observed through several successive groups of years; one apparent exception, however, being presented by the records for the years 1883-87. It will, then, be of advantage to consider this exception. First, let it be noted that after a series of years, marked by low mortality and heavy rainfall, the mortality has not risen in the next succeeding year of small rainfall to the height which it attains in the second succeeding year of small rainfall. For instance, the mortality was low for the years 1865-67. The next year was dry, and the mortality was doubled; and during the next year, which had about the same rainfall, the mortality was a second time doubled. So again, in the year after the wet period 1871-73 the rainfall diminished; and the mortality, though high in that, rose still higher in the next, year. Similarly, the mortality fell during the first of a series of wet years, and in some cases it fell still further during the second. Hence, it may be suggested that not only the rainfall of the year, but also that for prior years, has influence on scarlatina. If it be so, emphasis may be laid on the long continuance of heavy rainfall during the period 1878-82 (an average rainfall for the middle half of each year exceeding the averages for all the preceding groups of years, back to 1862), and on the possibility that this may have had to do with the want of increase of scarlatinal mortality in the period 1883-87. It may be remarked further that the mortality did rise in the last of these five years, in which the total rainfall was below the average, and again that it fell in 1888, when the rainfall was again largely increased.

It is not to be supposed that rainfall stands alone among meteorological elements. There are many others in intimate correlation with it, such as want of bright sunshine; and it will be observed that when the year as a whole was wet so also was the middle of the year, the second and third quarters, the period ordinarily of bright sunshine and heat, the period too when, as Messrs. Buchan and Mitchell's curves and Dr. Longstaff's show, mortality and sickness from scarlatina are on the ascendant.

There are, however, other matters which require to be considered

in this connexion. It may be asked what relation the death-rates from other diseases have borne to those from scarlatina: for, it might be that other diseases in wet years had modified the constitution of, or had been the cause of death of, persons who would otherwise have succumbed to scarlatina; or that in dry years other diseases had been unable to make headway, and so had left man a prey to the scarlatinal poison. I shall not attempt to give a complete answer to this question; but I may refer to some matters which bear upon it.

Turning to Chart I. it will be seen that the curve (curve VIII) of yearly death-rate from the principal zymotic diseases, minus scarlatina, shows that this latter disease is not the only one that has fallen off, taking groups of years together, since 1862; but it, also, shows that, whereas scarlatina has largely fallen off since 1875, the mortality from the other zymotics has been almost stationary since 1874. Let us now deduct from this curve the death-rates due to typhus, enteric, and simple and ill-defined fevers; and we shall obtain the curve VII. This curve shows that, putting aside the heavy mortality attendant upon cholera in 1866, and that upon small-pox in 1871, the fall of the curve VIII is due in chief part to reduction of deaths from typhus, enteric, and simple and ill-defined fever, which together have fallen from 12.8 for 1862, 9.9 for 1863, and 12.8 for 1864,—to 1.8 for 1885 and 1.7 for 1886, typhus having indeed disappeared from the mortality tables for 1886, and simple and ill-defined forms of fever being but poorly represented. If, again, deaths from small-pox also be excluded, the curve becomes still further flattened—as seen in curve VI. In the latter cognizance is taken of deaths from measles, diphtheria, whooping cough, diarrhoea, dysentery and cholera: and if, setting aside the remarkable death-rate from cholera in 1866, we take the mortality from the above diseases together, or from each of them singly, for small groups of years in succession, it will be found that there has been no great variation since 1862; though on the whole there has been a slight fall. The death-rates, however, from the zymotic diseases, other than scarlatina, rose enormously in the years 1866 and 1871, and somewhat in 1878, owing to cholera in the first year, to small-pox in the second, and to whooping cough in the third: and it may be asked whether the small scarlatinal mortality in these several years may be thus explained. It may have

been so in 1871; but it was not so, it seems, in the other years, for the scarlatinal mortality was almost as low in 1865 as it was in 1866, and it was lower in 1877 than it was in 1878. If, then, compensation as between scarlatina and other affections accounts for the variations in scarlatinal mortality, the fact still awaits demonstration.

It may, again, be asked whether the greater resort to the

TABLE Z.—RAINFALL (GREENWICH) AND SCARLATINAL MORTALITY (LONDON).

Annual rainfall in inches, decimals omitted.			Rainfall in 2nd and 3rd quarters in inches.		Deaths, per 10,000 of the population, from scarla- tina in London per year.	
1862	average 20.6	{ 26	average 10.6	{ 13	average 13.3	{ 12
1863		{ 20		{ 11		{ 17
1864		{ 16		{ 8		{ 11
1865	" 29.0	{ 29	" 15.3	{ 13	" 5.6	{ 7
1866		{ 30		{ 16		{ 6
1867		{ 28		{ 17		{ 4
1868	" 22.3	{ 25	" 8.3	{ 9	" 15.0	{ 9
1869		{ 24		{ 10		{ 18
1870		{ 18		{ 6		{ 18
1871	" 25.0	{ 22	" 13.0	{ 15	" 2.6	{ 5
1872		{ 30		{ 12		{ 2
1873		{ 23		{ 12		{ 1
1874	" 24.5	{ 20	" 11.0	{ 10	" 6.7	{ 7
1875		{ 28		{ 15		{ 10
1876		{ 24		{ 8		{ 6
1877	" 27.8	{ 26	" 15.6	{ 11	" 5.8	{ 4
1878		{ 29		{ 19		{ 4
1879		{ 31		{ 22		{ 7
1880	" 21.2	{ 29	" 10.0	{ 13	" 2.6	{ 8
1881		{ 25		{ 12		{ 5
1882		{ 25		{ 12		{ 5
1883	" 21.2	{ 21	" 10.0	{ 11	" 2.6	{ 5
1884		{ 18		{ 8		{ 3
1885		{ 24		{ 11		{ 1
1886	" 21.2	{ 24	" 10.0	{ 10	" 2.6	{ 1
1887		{ 19		{ 10		{ 3
1888		{ 27		{ 16		

Asylum Board's hospitals during late years for the isolation of scarlatina has had to do with the reduction of scarlatinal mortality in London. If we answer, yes, then, there remains the question why concurrently with the reduction of scarlatina in London there has been almost the same reduction in the provinces. Has isolation been provided for scarlatinal patients in the provinces in the same increasing proportion as it has in London? The Registrar-General

says it has not. Moreover, there is nothing to show that the provinces have been indebted for their scarlatina to introduction from London; for, as seen in Chart I, on each occasion since 1862, when the scarlatinal mortality began to rise in England, it either began, or rose higher, first in the country, e. g. in 1867, in 1873, and in 1878. It is at any rate clear that rainy seasons have been accompanied and followed for a time by reduction of scarlatinal mortality; and in being related to rainfall scarlatina is related also to other conditions attendant upon rainfall.

A few further remarks may now be made concerning this relation. A frequent and large total fall of rain, especially during the second and third quarters of the year, means, in addition, a clouded sky, deficiency of bright sunshine, inability of corn and grass to ripen with perfection, possibly of the scarlatinal virus also to acquire that phase (through scarcity or unsuitability may be of a necessary subordinate host) in which it can manifest the symptoms of scarlatina in man. It is not necessary that the relation of rainfall to scarlatinal mortality shall obtain in every locality. If the cow be an agent playing the part of an intermediate host, or other part, in the distribution of scarlatina in man, we may have to learn something of the breed of cow which plays such part most efficiently; since, from what has been recorded of the mortality from scarlatina in different parts of England, it would appear possible that the localization of particular breeds of cattle may have to do with variations of that mortality. In absence of all essential intermediary bearers, it would happen that though meteorological conditions were favourable, yet the relation of rainfall to scarlatinal mortality would not be apparent.

The cow's udder comes, as a matter of agricultural routine, into functional activity in spring, and remains in such activity for at least some months. If the cow be susceptible to a disease, which has one of its manifestations in an eruption of the udder; it is quite likely that that eruption will be most thoroughly developed, when the gland is in the condition referred to, than at other times. [In this connexion, there is the fact, as stated, that the vesicles of vaccinia in the cow in temperate regions are for the most part limited to the udder, while in more tropical latitudes they may be distributed generally over the body surface.] I am not supposing that virulence, or any special symptoms, should be

manifested in man at once from taking milk thus infected. It may be that the cow-virus, thus ready for dissemination in the human family, requires some continued transmission from man to man before it reaches its maximum capacity for mischief. As concerns, also, differences of scarlatinal mortality in different years, and their relations to atmospheric conditions, it may be borne in mind that higher vertebrates are not as homœothermal as they are generally said to be; and that their temperatures vary, especially in peripheral parts, very considerably with changes of external temperature (as I showed in the 'British Medical Journal' of July 1886), so that temperature of a certain degree and want of rain may stand in direct causal relationship to the varying phases of development of the virus in the cow's udder.

Much, however, that applies to the cow applies to man; moreover, if we allow that the virus of scarlatina may remain dormant for very considerable periods in the environment, outside its living hosts, it may be considered unnecessary to bring in lower organisms as hosts at all, the virus then behaving as most animal and vegetable forms do in obedience to certain atmospheric influences. As concerns the significance that attaches to the extra-animal environment there needs to be borne in mind the demonstration, obtained by Power at Hendon, of another host for the scarlatinal virus besides man; and in this connexion the observations of Professor Fodor need also to be borne in mind. A still more direct influence of rainfall on human scarlatina is that suggested by Dr. Longstaff (p. 191), an influence such as has been suggested also in the case of 'infantile diarrhœa.'

Though there is much that is speculative merely in the various propositions which have been brought forward as to indirect modes in which the scarlatinal virus may be influenced in its phases by meteorological conditions, there can be, I think, no doubt that variation in the constitution of this virus, however induced, does afford the most satisfactory explanation of the fact that the scarlatina, which I was observing in London, did actually vary in its ability to manifest itself after the fashion delineated in the foregoing sections.

While, therefore, allowing that the reaction of the individual to scarlatina varies largely with the constitution, the age, and the sex, of that individual; it seems that, none the less also, does it vary

with difference in the constitution of the virus itself at different times and seasons. It seems, moreover, that as a long succession of very dry years would be attended by a great reduction of sheep-fluke, and a long succession of very wet years might be attended by an extension of it (and *vice versâ* with the perfection of the cereals), so a long succession of dry years may favour scarlatinal depredations, and a long succession of wet years reduce them.

Such an interpretation is no more hypothetical than one which refers to meteorological condition the fact that fowls, on being taken from cold latitudes on one side of the equator to corresponding latitudes on the other side of the equator, do not lay, no matter how they be fed, except in the intervening warm or hot latitudes; or, again, the fact that in our own latitudes as the autumn advances the capacity for reproduction in the fowl declines, so that eggs are laid without shells, or frequently enough twin-yolked and incapable of developing into the chick.

It cannot be contended that explanation of phenomena such as these is more easy—more matter of course—than explanation of differences in the behaviour of microscopic organisms, when placed under different conditions as to light, heat, moisture, and so forth.

It is true that profound modification of the external characters and of the habits of higher organisms are effected only slowly, and by persistent influences operating through many generations; but we cannot say that modification, of profound importance at least as affecting their hosts, of beings infinitely lower in the scale of life may not be effected in a fraction of the time required in the case of higher organisms.

Let consideration be given to the suddenness with which sprouting occurs in the vegetable world, and to the inferences that have been drawn therefrom.

The observations of Dr. Mitchell (in 1849), of Nägeli, Billroth, Zopf, and those of Professor E. Ray Lankester (in 1876) are of especial importance in this connexion. Sir William Roberts, as already said, spoke, in 1877, of the non-pathogenic saprophyte as possibly passing almost *per saltum* into the pathogenic—into the ‘pathophyte’—and after no long interval as dying or reverting to its parent form. More recently, in 1884, Power, as a result of his investigation of the spread of infection from small-pox hospitals,

demonstrated, as regards Fulham hospital, a pronounced ability during May, and a relative disability during July, of the small-pox therein to infect people of the neighbourhood of that hospital; and he concluded his classic report to the Local Government Board on this subject by drawing attention to the circumstance that the dominant influence at Fulham, indicated by him as belonging to the very life of small-pox itself, was no new notion: 'For,' he said, 'some similar influences bound up with the life of small-pox cannot but have been suggested by the observed different ability of the disease at one and another time to communicate itself within doors to persons (under equal conditions of vaccination in the individuals), living in close and sustained relation with it; and, again, by the behaviour in London of small-pox, which after having attained by midsummer in certain years an epidemicity measurable by many hundreds of attacks per week, affording a totality of infectiousness capable, seemingly, of producing indefinite further increase of this disease, yet has as often declined suddenly, almost to extinction, only to rise again by slower degrees to fresh epidemicity in another winter season.'

So, also, variation of symptoms and of the morbid anatomy, to be observed in the case of certain other communicable diseases as prevalence of them extends and falls away—and of such diseases among lower animals I would in particular mention pneumo-enteritis of the pig—is most readily explicable on the supposition that it is dependent upon variation in the life-history of microbes concerned in their causation.

Inconstancy of symptoms, observed in the case of communicable disease, has, however, been referred to interfertilization of different kinds of microbes: it has, again, been thought due to supplemental invasions of the system by other microbes; such supplemental invasions having, it is said, been demonstrated in the case of scarlatina: and the phenomena, observed in my cases, may be thought of as being thus explicable.

As concerns the view which I have been myself inclined to take concerning these phenomena, I would in conclusion remark that it is as impossible to set aside indications, which are accumulating in abundance, as to varying phases natural to the life-history of microbes, as it is to set aside the broad teachings of evolution. Moreover, in the laboratory the morphological and

cultural characters of microbes, nay, even those of their properties which are commonly regarded as specific, appear likely to be found unstable quantities. Evidence is, in fact, already to hand to indicate that the 'epidemic constitution' of the older writers may have other than indefinite signification, that it will be found, in fine, to have that signification which I have above been contending for in the case of scarlatina.

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